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An Evaluation of The Massachusetts Work Experience Program

U.S. Department of Health and Human Services
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ABSTRACT

U.S. DEPARTMENT OF LABOR
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An Evaluation of the Massachusetts Work Experience Program

This study presents an evaluation of the Massachusetts Work Experience Program (MASSWEP) for men in the WIN unassigned recipient pool. The program began in January 1978 as a 15-month experiment. It was a multi-faceted effort to reduce the size of the unassigned recipient pool, including a two-stage screening of registrants to reassess their needs, the assignment of those found suitable to a work experience program, and an incentive to find regular employment by making available to these men the 30 and 1/3 treatment of earnings without the 100-hour limitation on work per month.

The study included a controlled experiment of the work experience component of the program as well as a process analysis of the whole program. The process analysis focused on the administrative procedures of the program, on the flow of clients through the steps of the program, and on the divergence of the actual program from what had been planned. The process analysis was the only tool for studying the two screening steps.

The controlled experiment was applied to those who had been found suitable for work experience in the screening. Of these, 14 percent were assigned randomly to a no-treatment control group; an additional 14 percent were to receive the 30 and 1/3 treatment of earnings without the 100-hour limitation, but not work experience; the remaining 72 percent would be assigned to work experience sites and would receive the 30 and 1/3 treatment if they found regular jobs. The work experience would require three days of work per week for thirteen weeks without pay.

As for results, neither of the two experimental treatments in MASSWEP had statistically significant effects on the rate of job finding. Although the raw data on job finding, when disaggregated by time and region, suggested some possibility that both of the treatments mattered, a variety of statistical tests showed that once other variables were controlled for, these differences were not statistically significant. What happened was that 57 percent of the Control Group found jobs without the MASSWEP treatments. The rates of job finding for the two experimental groups were slightly above this level, but not significantly so.

Neither of the two treatments led to a reduction in welfare payments among men in the experimental groups over the period of study. The total cost of MASSWEP amounted to $445 per client found appropriate for work experience. Since work experience produced no benefits in terms either of greater work or lower welfare payments, it was not cost effective in this program.

MASSWEP was not completely without benefit in that the screening process did help clean the MDES files of registrants who either did not belong in [illegible] or on welfare or who should have been receiving other WIN treatments. However, the program faced severe implementation difficulties including staff opposition, legal challenges, and organizational problems. Particularly in assigning clients to work experience sites and keeping them there, the program fell far short of [illegible] projections.

An Evaluation of

The Massachusetts Work Experience Program

by

Barry Friedman
Barbara Davenport
Robert Evans
Andrew Hahn
Leonard Hausman
Cecile Papirno

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Brandeis University
Center for Employment and Income Studies
Heller Graduate School
Waltham, Massachusetts
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The project was initiated by Merwyn Hans, Director of the WIN Program, and Gordon Berlin, Office of Research and Development, U. S. Department of Labor.

Only the authors are responsible for any deficiencies in the study.

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EXECUTIVE SUMMARY

This study presents an evaluation of the Massachusetts Work Experience Program (MASSWEP) for men in the WIN unassigned recipient pool.[1] The program began in January 1978 as a 15-month experiment. It was a multi-faceted effort to reduce the size of the unassigned recipient pool, including a screening of registrants to reassess their needs, the assignment of those found suitable to a work experience program, and an incentive to find regular employment by making available to these men the 30 and 1/3 treatment of earnings without the 100-hour limitation on work per month. MASSWEP was conceived following a period in which AFDC rolls had grown rapidly in Massachusetts as in the rest of the nation. Correspondingly, the WIN unassigned recipient pool--those registrants not assigned to any other WIN treatment--also was growing rapidly. While the unassigned recipient pool included many on a short term basis who were in transition between other treatments, evidence had accumulated that many registrants were remaining unemployed and in the pool for prolonged periods. The joint concern over the rising cost of welfare and the prolonged unemployment of some registrants led to a search for a program to deal with the problems of long term welfare recipients.

Massachusetts at first considered a workfare program outside of normal WIN regulations for men in the unassigned recipient pool on a

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[1] See Glossary and Technical Terms for explanations used throughout the report.

long term basis. Workfare would require men to work without pay to cover their welfare payment. After lengthy deliberation it was decided instead to use the work experience component of WIN. There would be a requirement to work without pay for thirteen weeks, three days a week, with two set aside for assisted job search. Not everyone would be required to participate in work experience. Rather, there would be a preliminary screening intended to identify those most appropriate for other WIN treatments. As finally adopted, MASSWEP included a series of steps involving first a screening process and second a controlled experiment centered around the work experience treatment.

Screening process

- Paper Selection: All men in the unassigned recipient pool were to be "paper selected" on the basis of their WIN record files to identify those likely to be suitable for work experience. Four criteria were used. If a file indicated that an individual was 1) unemployed for at least six months, 2) was registered with WIN for at least three months, 3) had been referred unsuccessfully to regular or CETA jobs, and 4) was unsuitable for referral to an alternative WIN component, then that individual was said to be paper selected and was to be scheduled for a special reappraisal interview for MASSWEP.

- Reappraisal: As a second step in the screening process, all those who had been paper selected were to be notified of the requirement to appear for a personal reappraisal interview. The reappraisal, to be conducted jointly or sequentially by MDES/WIN and MDPW/SAU personnel, was to determine whether or not an individual was suitable for assignment to a work site.

A person could be considered unsuitable for work experience if a physical or emotional problem warranted exemption from WIN: or if he first needed medical or social services, or needed them simultaneously with assignment to work experience but found them unavailable; or if other WIN components were more appropriate given his needs for employment services; or if he should be referred to vocational rehabilitation. Lastly, a judgment had to be made that the lack of recent work experience was inhibiting his re-employment.

The Controlled Experiment

- Assignment to Experimental Groups: The experimental design was to be applied to those judged "appropriate for assignment to work experience sites" in the reappraisal interviews. Immediately following a reappraisal interview, the WIN worker was to call Brandeis to obtain a random assignment of the client to one of the three groups, a control group and two experimental groups. Fourteen percent were assigned to the Control Group and were to receive no treatment following their reappraisal interview (other than any normal WIN actions). A second 14 percent were assigned to Experimental Group I; the 100-hour limitation rule was modified to allow them to receive the 30-and-1/3 treatment, but they would receive no work experience. The remaining 72 percent were assigned to Experimental Group II and were subject to both work experience and 30 and 1/3. During a second call-in, they were to be assigned to work sites.

- Work Experience Treatment: Individuals assigned to work sites were to work at those sites up to 24 hours per week, usually three days per week, and to spend the two other days searching, with the assistance of MDES/WIN, for regular jobs. A unique feature of MASSWEP was that work assignments were to be scattered in a variety of settings in private nonprofit organizations and state agencies, and in different low skill occupations rather than in group work projects. Jobs were to be tailored as closely as possible to the employment needs of individual clients. Training and work-related expenses (travel and lunch) and incentive payments of $30.00 monthly were to be given to men while on sites,

but there would be no wage payment. Those unable to find regular jobs during the 13 week work experience assignment were to be called in for a reassessment and a possible second work assignment. WIN adjudication procedures were to be available to all aggrieved clients.

Results

The study was intended to evaluate the screening mechanism as part of a process analysis. The work experience component was to be evaluated using the experimental design to determine its impacts on employment behavior and welfare experience. In addition the process analysis was to investigate problems related to the administration and operations of the program. The principal findings of the study may be summarized by the following points:

- The two screening processes identified large numbers of registrants in the WIN unassigned recipient pool who were not appropriate for work experience. With an initial unassigned recipient pool of 5228 (and more registrants added as the program proceeded), the number paper selected was 3120. The reappraisals found 1033 of these to be appropriate for work experience.

Considering those not appropriate, some were not long term unemployed; some found regular jobs or otherwise moved out of the unassigned recipient pool as part of the normal flux in welfare populations; some were sent to other WIN treatments because WIN staff considered these more appropriate for the client than work experience. In addition our process analysis found considerable unevenness across localities in the screening, with some offices apparently resisting actions that would lead to work experience assignments. We were unable to quantify the specific numbers taking each route, but the following qualitative statement can be made:

- The screening process identified many registrants who should have been receiving regular WIN treatments, but who had been left in the unassigned recipient pool instead.

Although the screening cleaned the unassigned recipient pool, it did not eliminate the need for such a pool. Without work experience, most of the 1033 men found appropriate for it probably would have remained in the unassigned recipient pool. Some of these could have been placed in regular WIN treatments, but the WIN staff felt that work experience would be more suitable for them. The WIN staff apparently felt that the expected payoffs from the standard WIN treatments would not be large for these men.

- We found no evidence that either the work experience treatment (Experimental Group II) or the modification of the 100-hour limitation alone (Experimental Group I) had a significant impact on the probability of finding a job.

The raw data on job finding, when disaggregated by time and region, suggested some possibility that both of the treatments mattered. After aggregating, a difference does appear, with 62.6 percent of those in Experimental Group II finding jobs, 55.8 percent of those in Experimental Group I and 57.0 percent of those in the Control Group. However, when other variables are controlled for statistically, no significant effect of either treatment emerged. If there was an effect, it must have been too small for our analytic tools to detect. Sample sizes were too small to test significant effects on other employment variables, but no prounounced effects were visable from comparing means.

- Follow-up instruments administered by Brandeis found job finding rates in all three experimental groups to be 17 to 20 percentage points higher than did data gathered through standard WIN forms.

The difference may arise because of slow communication between MDES/WIN and MDPW offices, or because of limited contacts between clients and MDES/WIN offices, or because of a high rate of concealing work from MDES/WIN among AFDC-UF clients. Whatever the reason, work data available to MDES/WIN is likely to be unreliable, substantially understating the amount of work among WIN registrants.

- The majority of men considered appropriate for work experience were employable and capable of finding work through normal channels. They are much slower than average in finding jobs, but their lengthy bouts of unemployment are not a barrier to finding jobs; they can do so without the special MASSWEP.

This finding follows from the fact that 55 percent of the Control Group found jobs without either of the MASSWEP treatments. Of course, the remaining 45 percent did not over a period of at least 15 months. It is likely that these include a hard core of essentially unemployables. The work experience treatment made little difference since most job finders would have found jobs anyway while the hard core unemployables appear not to have been changed by their experience.

- Neither of the two treatments tried in MASSWEP produced significant reductions in welfare payments.

Welfare payments could go down either because the person worked more or because the person was intimidated off welfare by MASSWEP. We already observed that there was no significant effect of MASSWEP on job finding. Since there was also no significant effect on welfare payments, it seems likely that MASSWEP had little intimidation effect.

- The cost of MASSWEP to the federal WIN program and the state combined was $269,592. Accounting for the CETA workers used in running the program, the total cost was $459,592. This amounts to $445 per client found appropriate for work experience.

- With no significant benefits from work experience either in terms of increased work or reduced welfare costs, the work experience treatment was not cost effective. It is likely that the screening process produced some benefits, but we were unable to quantify them in dollars.

Although we found no significant effects of mandatory work experience on job finding or welfare costs, the MASSWEP experiment did not provide a perfect test since so many problems arose in implementing and administering the program. Some of the principal shortcomings are summarized below:

- An administrative review of the two screening processes combined found an error rate of 10 to 13 percent. Our process analysis found considerable unevenness and slackness in performing the screening, particularly in the early stages of the program.

- The modification of the 100 hours limitation did not operate smoothly in that many of those eligible for 30-and-1/3 were not informed of this fact or were not informed of its significance, particularly early in the experiment.

- Of the 1033 men found appropriate for work experience, our experimental design selected 754 randomly to be subject to this treatment. Of these, 256 ever appeared at designated work sites. While only 27 spent the planned 13 weeks at their work site. Many spent only one or two days at their assigned work sites. Many who did not go to work sites did find jobs on their own. The assignment to a work site may also have "smoked out" some clients who already had jobs. However, many others were simply never assigned.

Policy Considerations

In view of our research findings we may now address two central policy questions: 1) Is mandatory work experience--or similarly workfare--an appropriate treatment for its targeted population or are there more suitable treatments for these clients? 2) Given that mandatory work experience did not function well in MASSWEP, could it be made to operate more satisfactorily in other places?

Consider first the suitability of mandatory work experience for its target population. There are two approaches to mandatory work programs. The workfare approach would assign virtually all registrants to work sites. In contrast, mandatory work experience could be viewed as an assistive service: men long unemployed might need work experience to establish the credentials and work habits necessary to be considered for regular jobs. Considered as an assistive service, work experience may be appropriate for some clients while others would benefit more from other services. Although there was some confusion about the objectives of MASSWEP, it more or less followed the assistive approach, with the screening used to assign clients to other WIN treatments if these would be more beneficial. Our results allow us to draw some conclusions concerning the size of the target population suitable for work experience as an assistive service.

- In the initial planning stage for MASSWEP the size of the likely target population was greatly exaggerated. It was originally expected that the screening processes would find about 2800 men suitable for work experience, but only 1033 were actually found once all the inappropriates were screened out. Given the irregularities in the screening process, the 1033 figure may be too low, but it is likely that the true figure would also be less than early expectations.

Overestimation of the likely numbers has occurred in other workfare programs as well. Apparently, advocates of workfare tend to exaggerate the potential need for such a program. The underestimation by MDES of the extent of work among registrants probably helped contribute to this exaggerated impression of the need for work experience. Indeed any program with irregular and infrequent contact with clients--as certainly was the case in the unassigned recipient pool--will be able to provide only very imperfect information on the work effort of its clients.

- Although the screening processes found 1033 clients more suitable for work experience than for any existing WIN treatment, this appears to have been an erroneous judgment in many cases, at least if work experience is viewed as an assistive service. The screening process, like the planning stage, overestimated the number of men who could benefit from work experience.

Fifty-seven percent of men in the Control Group--all long-term unemployed--found jobs without any work experience treatment. We may conclude that the long unemployment spells of these men did not constitute an insurmountable barrier to employment. For such men, work experience is likely to provide fewer benefits than other assistive services. If there were a need for work experience, and even this is dubious in view of our results, it must have been concentrated among

the remaining 43 percent, about 445 men. This reinforces the idea that the need for work experience was greatly overestimated in planning for MASSWEP.

- The number of men who would benefit from a well-functioning work experience program is limited. Presumably, it is just some subset of the 45 percent who were not finding jobs. Voluntary work experience for women has been shown to yield benefits. However, most of the men in our target group have a fair amount of labor market experience in contrast to the women who voluntarily participate in work experience.

Considering the job finders in the Control Group, they were very slow in finding work, although they were not unemployable. What they needed would be a treatment to speed their return to work. Besides its assistive aspect, work experience could have intimidated some into finding work sooner, although this effect seems to have been minor in MASSWEP. However, would any alternative work better?

- For the men who were finding jobs anyway, but very slowly, we may speculate that the regular WIN treatments would have been as effective as any other. The trouble with these treatments is that men in the target population were not receiving them. These men had work problems and probably could have benefited from the combination of pressure and assistance offered by WIN. But to be effective, MDES would need regular and frequent contact with these men, something that had been lacking.

- Although mandatory work experience is likely to have only limited effect in assisting men to overcome labor market problems, it may have appeal as a punitive measure. A strictly enforced program will either intimidate registrants into

leaving welfare altogether or will force them to work without pay at special jobs. From the point of view of welfare costs, there will be savings to the extent that men are intimidated off of welfare, but not to the extent that they go to work experience sites without benefiting.

Consider now the possibilities that a mandatory work experience program might operate more successfully than MASSWEP. We know that voluntary work experience for women has yielded benefits; also, some workfare programs have functioned over prolonged periods, although there have not been rigorous evaluations of their effects on work and on welfare costs. Some of the difficulties with MASSWEP were implementation problems associated with its introduction. But some could lead to ongoing complications.

- Mandatory work experience and workfare are sensitive political subjects with strong feelings on both sides. There was extensive lobbying in the planning stage of MASSWEP. There were legal pressures and even an injunction. The courts are now more sensitive to issues of due process in mandatory programs. Differences in political views among WIN staff contributed to some of the irregularity in what happened across local offices. These problems of political conflict are likely to appear in any new workfare program.

Although voluntary work experience is inexpensive to operate and may appear to offer a cheap alternative to other WIN treatments, the mandatory element will make such a program more expensive and more complicated to run. The following four items spell out some sources of higher cost.

- A mandatory activity requires frequent contact with clients together with monitoring of their activities. This can be both expensive and complicated, to the extent clients try to evade the intent of the program.

- For any work experience program, work sites must be chosen carefully. In MASSWEP some clients left work sites quickly because they did not want to work, but in many cases the work sites were just not suitable. Developing sites and assigning clients to suitable ones can be costly.

- Men sent to work sites on a mandatory basis can pose problems for the work sponsor in terms of discipline and productivity. If there are implicit (although not direct monetary) costs to work sponsors, they may be reluctant to participate, adding to the costs of the MDES or similar agency for job development.

- In a mandatory program, adjudication procedures must come into use whenever a client fails to comply with program requirements. To insure due process, these procedures are slow and complicated. If sizable numbers of clients do not comply with rules, adjudication costs can become substantial.

In MASSWEP there was a failure to develop sites carefully and to deal with clients who left work experience sites early. Had these problems been dealt with, the others might have also appeared. Nevertheless, it is conceivable that with sufficient resources and administrative control a mandatory work experience program could be made to function. Whether it would be more successful than MASSWEP in increasing employment in regular jobs or in reducing welfare costs is problematical, but our previous arguments suggest that the number of potential gainers is small. At the same time, the cost of an effective program is likely to be higher than that of MASSWEP.

The basic problem is that getting people to work in regular jobs and thereby reducing welfare costs is expensive. Whether considering assistive services or coercive pressures, it is not clear that we are prepared to spend--or even ought to--all the resources necessary to achieve success. Mandatory work experience in particular is likely to benefit such a small population that even a smoothly functioning program probably would not be cost effective. The particular program tried in MASSWEP certain was not.

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GLOSSARY
and
TECHNICAL TERMS

AFDC-UF.....Aid For Dependent Children-Unemployed Fathers

APA.....Administrative Procedures Act

CETA.....Comprehensive Employment and Training Act

CWEP.....California Community Work Experience Program

DOL.....Department of Labor

EOEA.....Massachusetts Executive Office of Economic Affairs

ESARS.....Employment Service Automated Reporting System

HEW.....Department of Health, Education, and Welfare

MASSWEP.....Massachusetts Work Experience Program

MCAW.....Ad Hoc Massachusetts Coalition Against Workfare

MDES/WIN.....Massachusetts Division of Employment Security Work Incentive Program

MDPW.....Massachusetts Department of Public Welfare

MINNWEP.....Minnesota Work Equity Project

OJT.....On the Job Training

PSE.....Public Service Employment

SAU.....Separate Administrative Unit

UI.....Unemployment Insurance

WIN.....Work Incentive Program

continued...

GLOSSARY
and
TECHNICAL TERMS
(continued)

AFDC (Aid for Dependent Children) is a program authorized by Title IV-A of the Social Security Act to provide financial assistance and social services to needy families with children.

AFDC/UF (unemployed fathers) is the component of AFDC for intact families including a father who is employable but who is unemployed or working no more than 100 hours per month. About 25 states have elected to provide this assistance.

WIN (Work Incentive Program) is an employment-oriented program for that part of the AFDC population considered employable. All AFDC applicants and recipients, unless exempt by law, must register with WIN.

Registrant - An AFDC applicant or recipient who has registered with the WIN program for manpower services, training, and employment.

30 and 1/3 Income Disregard - If a qualified AFDC recipient becomes employed, the first $30 total monthly gross earned income plus 1/3 of the remainder (gross earned income minus $30) is disregarded (i.e. deducted) in computing the amount of the AFDC assistance payment.

100-hour limitation - Families in AFDC-UF are covered under the 100-hour limitation instead of the 30 and 1/3 income disregard. If the father obtains steady, regular employment of 100 hours or more per month, the family is no longer eligible for AFDC assistance.

CHAPTER I

Introduction

This study presents an evaluation of the Massachusetts Work Experience Program (MASSWEP) for men in the WIN unassigned recipient pool. The program began in January 1978 as a 15-month experiment. It was a multi-faceted effort to reduce the size of the unassigned recipient pool, including a screening of registrants to reassess their needs, the assignment of those found suitable to a work experience program, and an incentive to find regular employment by making available to these men the 30 and 1/3 treatment of earnings without the 100-hour limitation on work per month. The program was conceived following a period in which AFDC rolls had grown rapidly in Massachusetts as in the rest of the nation. Correspondingly, the WIN unassigned recipient pool--those registrants not assigned to any other WIN treatment--also was growing rapidly. While the unassigned recipient pool included many on a short term basis who were in transition between other treatments, evidence had accumulated that many registrants were remaining unemployed and in the pool for prolonged periods. The joint concern over the rising cost of welfare and the prolonged unemployment of some registrants led to a search for a program to deal with the unassigned recipient pool.

Problems similar to those in Massachusetts existed in other states, too, and produced similar efforts to find new programs. One response to these problems has been a growing interest in "workfare" programs. Under workfare, clients who cannot find regular jobs typically are required to

"work off their welfare payments" in public service jobs. The amount of work required of the client is calculated on the basis of the minimum wage. Thus, if the client's monthly benefit is $350 and the minimum wage is $3.50, he is required to work $\frac{350}{3.50}$ = 100 hours without any pay other than his benefit payment.

Massachusetts did consider a workfare program, but in the end opted for something different. A workfare program, if it succeeds, will get people to work, but it may not help much to reduce program costs. Clients will work at their public service jobs, but they will continue to draw the same benefit payments. Only if clients are intimidated by workfare into leaving welfare altogether will it reduce welfare costs. Officials in Massachusetts were concerned with the difficulties some registrants had in finding work in addition to the fact of unemployment. After lengthy consideration, it was decided to fit the new Massachusetts experiment, into the WIN program with its tradition of mixing pressure to work with assistive services. Rather than the typical workfare concept, Massachusetts would now make use of the work experience component of WIN for men in the unassigned recipient pool.

The work experience component of WIN involves 13 weeks of work in a public service or non-profit agency without pay. It had been used for women in Massachusetts on a strictly voluntary basis, but not for men. MASSWEP introduced it for men in the unassigned recipient pool who were found suitable in the screening process. These men would be required to participate. The requirement of participation together with the absence of pay make work experience similar to workfare. On the other hand the amount of work required was not calculated individually on the basis of

the client's welfare payment. There was a standard requirement that the registrant work three days a week for 13 weeks. The remaining two days a week were reserved for search for regular employment, with assistance from WIN workers.

While not identical with workfare, the MASSWEP program addresses the same problems and has some similarities to it. Results of this experiment thus should be of interest nationally to the extent that workfare or similar programs receive further consideration. This study was intended to investigate several broad questions:

1. Was MASSWEP administratively feasible?
2. Did MASSWEP succeed in increasing employment in regular jobs among registrants in the unassigned recipient pool?
3. Did MASSWEP succeed in reducing welfare costs associated with registrants in the unassigned recipient pool?
4. Was MASSWEP cost effective?

In order to investigate these questions a controlled experiment was conducted in which some registrants received the full MASSWEP treatment while others received only partial treatments. Controlled experiments are particularly important in studying the impacts of work programs on welfare recipients. By now numerous studies have shown that welfare recipients do work in large numbers even though their work experience may be intermittent or irregular. Even without program treatments, welfare receipients tend to find jobs and move off welfare, although they often return later. The movements have been described as cycling in and out of jobs and correspondingly off and on welfare. In view of such normal cycling behavior, the finding that a program has a high placement rate cannot be taken as a measure of success,

since some of the jobs would have been found anyway. It is only through controlled experiments that programmatic effects can be distinguished from what would have happened anyway. This project was able to test the key treatment elements of MASSWEP through a controlled experiment.

This chapter will now describe in more detail the MASSWEP program and the associated controlled experiment, then it will briefly introduce what will be discussed in the rest of the study.

A. Description of MASSWEP

Figure I summarizes the steps of the MASSWEP program through which clients moved. The screening process involved two steps, paper selection and reappraisal, and was intended to identify those suitable for the work experience treatment. The controlled experiment was applied only to those suitable for this treatment. Consider now each of the steps shown in Figure I.

1. Paper Selection

All men in the unassigned recipient pool were to be "paper selected" on the basis of their WIN record files. The paper selection was in effect a way of cleaning the files while identifying registrants who seemed appropriate either for work experience or other WIN treatments which they were not then receiving. Four criteria were used to identify registrants likely to be suitable for work experience. If a file indicated that an individual was 1) unemployed for at least six months, 2) was registered with WIN for at least three months, 3) had been referred unsuccessfully to regular or CETA jobs, and 4) was unsuitable for referral to an alternative WIN component, then that individual was said to be paper selected and was to be scheduled for a special reappraisal interview for MASSWEP.

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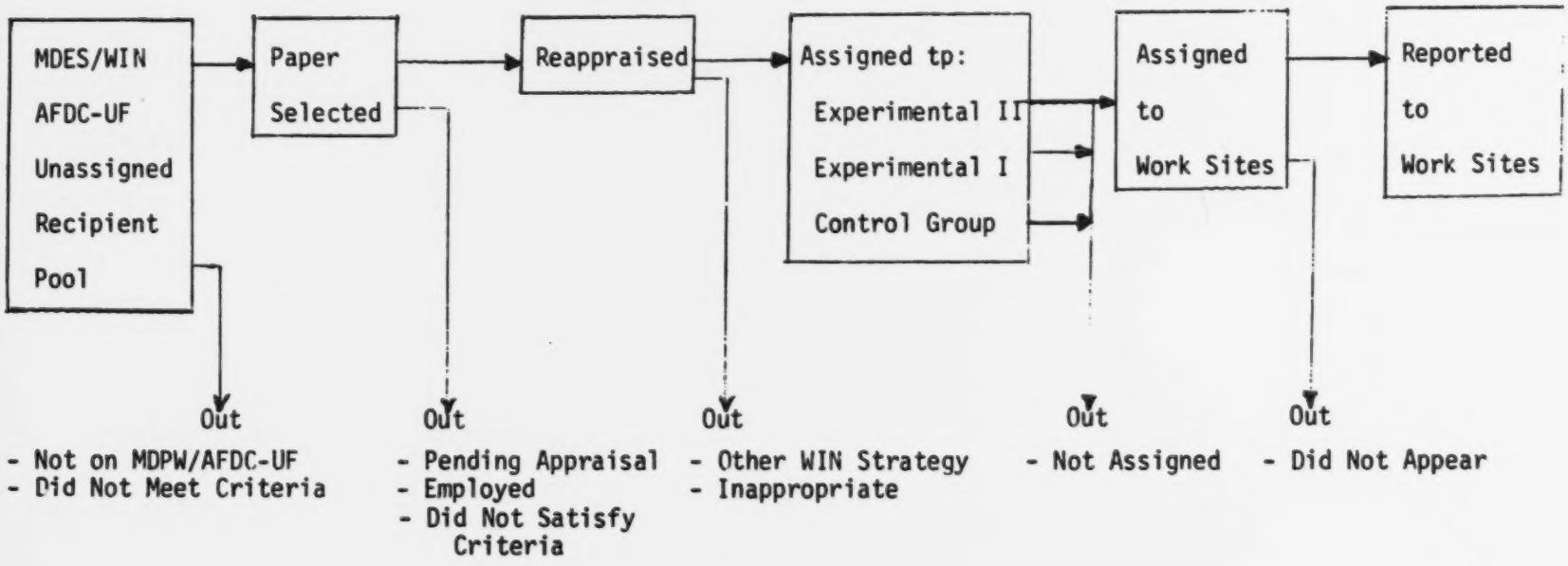
F I G U R E I

MASSWEP IN STAGES: CLIENT FLOW FROM PAPER SELECTION THROUGH APPEARANCE AT WORK SITE



2. Reappraisal

As a second step in the screening process, all those who had been paper selected were to be notified of the requirement to appear for a personal reappraisal interview. The manner of notifying clients for reappraisals was left to the discretion of the local WIN teams throughout the state. The reappraisal, to be conducted jointly or sequentially by MDES/WIN and MDPW/SAU personnel, was to determine whether or not an individual was suitable for assignment to a work site. A person could be considered unsuitable for work experience if a physical or emotional problem warranted exemption from WIN; or if he first needed medical or social services, or needed them simultaneously with assignment to work experience but found them unavailable; or if other WIN components were more appropriate given his needs for employment services; or if he should be referred to vocational rehabilitation. Lastly, a judgment had to be made that the lack of recent work experience was inhibiting his re-employment. By most standards, it can be said, the program was designed to give WIN and SAU personnel substantial discretion in case management regarding suitability for assignment to work experience. Indeed, assignment to a work experience site was only one of a number of possible outcomes of the two stages of screening. More broadly, the screening process might be seen as seeking the most suitable treatment for the long term unemployed in the unassigned recipient pool.

3. Assignment to Experimental Groups

The experimental design was to be applied to those judged "appropriate for assignment to work experience sites" in the reappraisal interviews. The purpose of the experimental design was to test the work experience

treatment against a no-treatment control group. However, those subject to the work experience treatment were also given an incentive to seek regular employment in that the 100-hour rule was waived for them while the 30-and-1/3 rule would apply to their earnings.[1] To separate the effects of the work experience treatment from the 30-and-1/3 rule treatment, it was decided to set up a second experimental group receiving only the 30-and-1/3 treatment, but not work experience. There was thus one control group and two experimental groups.

Immediately following a reappraisal interview, the WIN worker was to call Brandeis to obtain a random assignment of the client to one of the three groups. Fourteen percent were assigned to the Control Group and were to receive no treatment following their reappraisal interview (other than any normal WIN actions). A second 14 percent were assigned to Experimental Group I to receive the 30-and-1/3 treatment, but no work experience. These people were to be called in again after the reappraisal interview to be notified that the 100-hour rule was being waived and that the 30-and-1/3 rule would apply to all their earnings throughout the course of the 15-month experiment. The remaining 72 percent were assigned to Experimental Group II and were subject to both work experience and 30 and 1/3. During a second call-in, they were to be assigned to work sites. In addition they were to be told that if they found regular employment, they would have the 100-hour rule waived. Individuals assigned to work sites were to work at those sites up to 24 hours per week, usually three days per week, and to spend the two other days searching, with the assistance of MDES/WIN, for regular jobs.

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[1] Technically, the 100-hour rule was not waived. In terms of funding, HEW granted permission to use Section 1115(a)(2) for this experiment which allows Federal matching funds to be used for the extra welfare costs which otherwise would not be included as expenditures under 403. In other words, the Federal government agrees to match state funds for costs of over the 100 hours limitation. In terms of what happens to clients, it is as if the 100 hours limitation was replaced by 30 and 1/3.

4. Work Sites

A unique feature of MASSWEP was that work assignments were to be scattered in a variety of settings in private nonprofit organizations and state agencies, and in different low skill occupations rather than in group work projects. Jobs were to be tailored as closely as possible to the employment needs of individual clients. Training and work-related expenses (travel and lunch) and incentive payments of $30.00 monthly were to be given to men while on sites, but there would be no wage payment. Those unable to find regular jobs during the 13 week work experience assignment were to be called in for a reassessment and a possible second work assignment. Lastly, WIN adjudication procedures were to be available to all aggrieved clients.[2]

B. Plan of Study

The study will involve an impact analysis and a process analysis. The impact analysis is concerned with the effects of the experimental treatments--work experience and 30 and 1/3--on client employment behavior, welfare benefits, and program cost. The impact analysis relies on the experimental design which permits comparisons of these effects between the control group and the two experimental groups. Of course, statistical controls will be used in making the comparisons.

The process analysis is needed to study the parts of MASSWEP not subject to experimental variation, as well as to evaluate the experimental treatments in terms of whether they operated as planned. Matters like the procedures for screening registrants in MASSWEP, the administrative structure,

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2 MDES/WIN, Work Initiative Regulations, effective April 11, 1978.

and the political and legal pressures surrounding the program were not subject to experimental treatments. The process analysis will study these aspects of the program. As for the experimental treatments, it is by now a familiar fact that programs often perform differently than planned. The actual experimental treatments will be evaluated by the process analysis to see how they differed from planned. In particular the process analysis will examine variations in treatments across clients. In addition to revealing the difficulties in implementing a new program, this part of the process analysis should contribute to the impact analysis. To the extent that unplanned variations in treatments can be measured, they can be introduced as control variables in the impact analysis. Otherwise, the process analysis is needed to identify limitations in the impact analysis.

Chapter 2 will review the literature on a variety of programs having similarities to MASSWEP to see what is already known.

The process analysis is presented in Chapters 3 and 4. Chapter 3 will review the planning and development of MASSWEP, concentrating on the political and legal pressures faced and how they affected the program that actually emerged. Chapter 4 traces the client flow in detail in terms of what actually happened at each step and how this compared to plan.

Chapters 5 and 6 present the impact analysis. Chapter 5 deals with the impacts on employment behavior. A number of measures are used, although the most reliable probably is the placement rate. Chapter 6 evaluates impacts on welfare payments and presents a cost benefit analysis of the program.

CHAPTER II

Review of Literature

A number of work programs have been tried including both variants of workfare and of work experience. In this study we are interested primarily in mandatory work programs. Workfare is one type. On the other hand the work experience component of WIN often has been voluntary, although it was mandatory for the men in MASSWEP. This chapter summarizes evidence from past efforts, concentrating on three principal topics: 1) the feasibility of mandatory work programs, 2) the normal work and welfare patterns of welfare recipients not subject to programs like workfare and the difficulties these create for estimating the pure effects of mandatory work programs, and 3) impacts of work programs on work and welfare behavior. In addition the chapter will consider what is known about the second experimental variation in MASSWEP--the 100 hours limitation.

A. Evidence on the Feasibility of Work Programs

The record is mixed on whether mandatory work programs can operate smoothly. The workfare program in Cincinnati has operated for over 40 years, while some recently started programs have encountered problems in being implemented. The start-up problems in new work programs can be severe as is illustrated by two cases: the California Community Work Experience Program (CWEP), a workfare program which ran for three fiscal years starting with fiscal 1973, and the Minnesota Work Equity Project (MINNWEP), a welfare-related work program which enrolled its first clients in July 1978. In CWEP, a variety of court decisions and the threat of an injunction made

administrators cautious about enrolling clients and made whole counties unwilling to start the program until well into the second year of the project. The California legislature, after voting to abolish CWEP--a bill later vetoed by the Governor--required that a specified proportion of slots in the WIN Program had to be filled before AFDC and AFDC-UF recipients could be required to enter CWEP. Other employment and training efforts, such as the Public Employment Program and the California Employables Program, also competed for the involvement of welfare recipients, thus diminishing the possible impact of CWEP. The unanticipated disruptions and competing efforts resulted in "...an off-and-on approach to [CWEP that] neither built enthusiasm in responsible staff, nor caused user agencies to look upon CWEP as a reliable source of workers."[1] Moreover, pressures from public groups led county welfare administrators in some instances to refuse to implement CWEP throughout its existence. Moreover, in counties where the program was implemented, the staffs in field offices often ran CWEP as a voluntary program, being "...as highly selective in their screening for CWEP referrals as they would be in selection for training or referral to regular jobs."[2] In some of the counties where initially the staff did implement the program, a change in the state administration led to a breakdown in the program because it was assumed that the new Governor did not support CWEP.

Similarly, MINNWEP faced unanticipated constraints, leading evaluators to note that intended participants were excluded and that the program never

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1 State of California, Employment Development Department, Third Year and Final Report on the Community Work Experience Program, DHEW, Office of Research and Demonstration Projects, April 1976, p. 10.

2 Ibid., p. 11.

developed its "innovative character."[3] Instead of a program requiring work, the program quickly came "...to resemble a Work Incentive Program run by CETA prime sponsors. The emphasis...shifted away from minimum-wage jobs toward prevailing wage jobs and job training"[4] and has continued to be a prevailing wage program, with emphasis on training and a rich mix of services, somewhat like WIN I. Similar to CWEP, entire counties chose not to participate in the program. Because of interagency conflict, the opposition of staff in the responsible agencies, and public opposition, MINNWEP was scaled down drastically from what was planned.

In contrast to these constrained, altered, and unimplemented work programs, there have been work programs which have functioned smoothly over prolonged periods. In Cincinnati, for example, workfare in General Assistance has functioned for more than 40 years. In early 1978, roughly 1900 individuals worked at 28 workfare sites, at an implicit wage rate that was about three-fourths of the federal minimum. Movement through the program is substantial. Roughly 300 to 500 apply monthly for General Assistance and about 300 to 400 leave this program each month. Four times as many are assigned to work sites as appear for work, with those not appearing risking the loss of their benefits. Thus, the program seems both to induce

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3 MINNWEP required work of welfare clients, but clients benefited financially as some earnings are added to disposable income. MINNWEP, therefore, is not a workfare program.

4 M. G. Trend, et al., Executive Summary of the Final Implementation Report for the Minnesota Work Equity Project, submitted by Abt Associates to the U.S. Department of Labor, December 15, 1978, pp. 1-3. While some of the indicated problems are attributable probably to the experimental nature of MINNWEP, a principal source of the difficulties was the program's work requirement.

departures from welfare and to get people to work sites.[5] Similarly, in the Work Relief Employment Program related to General Relief in New York City, evidence of regular and substantial enrollments is available, with many placements in regular jobs in the New York City government.[6]

As one final illustration of a functioning program, in the relatively well publicized workfare program in Utah, 8987 WIN registrants were screened by the welfare department between June 1974 and June 1976. Of these, 1850 registrants were assigned to workfare. Of the 1850, 609 appeared at work projects for some unspecified period of time. Part of the fall-off appears to result from the intimidation of clients by workfare.[7]

If there is a lesson to be learned from these experiences that might apply to MASSWEP, it is that a relatively short term experiment with a work program like MASSWEP should be expected to have serious implementation problems and might operate in fact far differently than anticipated. Over the long run, however, at least some types of mandatory programs have been shown to be feasible.

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[5] Robert Fodor and Jay Rowen, Work Relief: Search for a Positive Position, U. S. Department of Labor, Office of Work Incentive Programs, draft discussion paper, June 6, 1978, pp. 15-16.

[6] Lieberman Research Suburban, An Evaluation of the Work Relief Employment Project in New York City, prepared for the New York Department of Social Services, January 1975.

[7] Utah Department of Social Services, Office of Evaluation, Review of the Work Experience and Training (WEAT) Program, April 1977, pp. 3, 9, and 10.

B. Evidence on the Normal Work Patterns of Welfare Recipients

In addition to the question of whether work programs can function, there is the question of whether in functioning they succeed in their objective of reducing welfare costs and increasing work effort. Previous studies of work programs often have not evaluated their impacts on costs and work effort adequately. In the Utah study, for example, a claim is made that since 909 of 1850 AFDC clients assigned to workfare "became unavailable" for workfare assignment, payments that would have been made to them are savings in welfare that resulted from workfare.[8] Whether this reflected a normal flow of recipients from joblessness into employment and off of welfare is impossible to tell because no control group, actual or statistical, is available for comparison. Thus, although claims of this nature are made repeatedly in various reports, they cannot be distinguished by the available information from the alternative claim that what happened would have happened anyway even without the work program.

A large number of studies on the work and welfare patterns of AFDC recipients suggest that some number of recipients would enter jobs and leave welfare even in the absence of workfare. Hausman and Friedman note, for example, that at any point in time in 1973, 16 percent of AFDC mothers and 12 percent of AFDC-UF fathers were employed while receiving welfare. An additional 45 percent of such AFDC mothers

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[8] Ibid., p. 9.

and 55 percent of AFDC-UF fathers worked sometime during the two years prior to that point. On the welfare side, these researchers report on several studies which yield estimates of the average length of stay on AFDC-UF and AFDC. Depending on a number of factors like state, welfare category, time and whether the data were cross-sectional or longitudinal, these estimates vary between .5 and 4.5 years. In sum, these authors review a substantial number of studies of flows between joblessness and employment as well as between welfare and non-welfare status, all of which substantiate sizeable flows between these statuses in various periods during the last 15 years.[9] Lyon, in his review of this literature, comes to the same conclusion.[10] Lastly, a study in 1977 of more than 11,000 WIN registrants in 15 states revealed that 80 percent of male and 27 percent of female registrants left the WIN Program within 12 months following registration.[11] The point is, then, that to establish that workfare, or any other intervention, has an effect on the rates of entry into jobs and exits from welfare, one must use a technique that allows one to compare these rates with those that are normal or expected.

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9 B. Friedman and L. Hausman, Work and Welfare Patterns in Low Income Families, a monograph submitted to the U.S. Department of Labor, June 1975, Chapters II and III.

10 David W. Lyon, The Dynamics of Welfare Dependency: A Survey, Ford Foundation Welfare Policy Project, Institute of Policy Sciences, Duke University, Spring 1977.

11 P/RA Research, Inc., An Analysis of Unassigned Recipients/Registrants in the WIN Program, a monograph submitted to the U.S. Department of Labor, Employment and Training Administration, April 26, 1978, p. 3; and another recent paper on the movements between various employment and welfare statuses is: P. Robins and N. Tuma, Changes in Rates of Entering and Leaving Employment Under a Negative Income Tax Program: Evidence from the Seattle and Denver Income Maintenance Experiments, Stanford Research Institute, March 1977.

MASSWEP is designed to deal with long-term registrants in the unassigned recipient pool who may have greater difficulties than average in cycling off welfare and into work. Even for these registrants, however, returns to work are possible. Thus the same methodological concern with identifying normal returns to work applies to a study of MASSWEP as well as to other programs. We were fortunate in having a controlled experiment where normal work patterns would be observed for the control group.

C. Evidence on the Impacts of Work Programs

Our principal interest is in the impacts of mandatory work programs. MASSWEP used the work experience component of WIN on a mandatory basis. However, the work experience component has been used on a voluntary basis, mainly for women. A recent study by Ketron found that women who went through work experience had subsequent earnings significantly higher than those who had not. Indeed, since the costs of work experience were relatively lower than those of other WIN components, it was estimated to be the most cost effective WIN component.[12]

As for mandatory programs, only very gross and crude measures of their impacts on employment and welfare caseloads are available at this time. In the study of CWEP, rates of welfare case closings because

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12 Ketron, Inc., The Long-Term Impact of WIN II: A Longitudinal Evaluation of the Employment Experiences of Participants in the Work Incentive Program, January 1980, pp. 91-95, 135-138.

of employment in the combined AFDC and AFDC-UF populations were compared in counties with CWEP and without CWEP. The rate of case closings because of job finding dropped over time both in counties with and without CWEP between the pre-CWEP and CWEP periods, but the differences in these changes over time were not statistically significant. Further, the number of welfare applications grew more rapidly over time in the CWEP as compared to the non-CWEP counties, and these differences were statistically significant.[13] The latter could be evidence of welfare "churning" induced by workfare: induced departures, to the extent they existed, in CWEP counties may have induced early returns to welfare.

A very crude estimate of the impact of the workfare program in Utah can be obtained by comparing the rates of growth over time in its AFDC and AFDC-UF caseloads with those of neighboring states and of the nation. The Utah workfare program began in June 1974, so we can compare rates of growth in AFDC and AFDC-UF caseloads among Utah, Colorado, Nevada, and the nation between January 1974 and January 1979. (Nevada was chosen as a state geographically close to Utah; Colorado was the state geographically closest to Utah that had an AFDC-UF program. Neither Nevada nor Colorado had a workfare

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[13] Op. Cit., State of California, pp. 23 and 24.

program in AFDC or AFDC-UF.) As can be seen in Table II-1, the growth rate in the Utah AFDC program during the period exceeded that for the country as a whole and far outstripped those for neighboring states. In AFDC-UF, the caseload dropped dramatically in Utah compared to the country but exceeded only slightly the decline in the AFDC-UF caseload in Colorado. Obviously, these comparisons, like those presented for CWEP, are crude because they do not control for the other factors that may be influencing changes in caseloads. The comparison with Colorado, however, does suggest that the fall in the AFDC-UF caseload could be a normal phenomenon rather than something caused by workfare.

Table II-1

Rates of Change in Caseloads in AFDC and AFDC-UF in Several States and in U.S., January 1974--January 1979[a]

	AFDC (Percent Change)	AFDC-UF (Percent Change
Colorado	0.0	-20.4
Nevada	-19.3	(no program)
Utah	+16.0	-22.1
U.S.	+ 9.6	+10.7

a/ Source: U.S. Department of Health, Education, and Welfare, National Center for Social Statistics, Public Assistance Statistics, January 1974 and January 1979, Tables 4 and 5.

If a work program does achieve a short run success, that does not assure that its long run effects will also be favorable. Evidence is slight, but some long run problems are at least possible. For example, if

people leave welfare quickly to take jobs in order to avoid workfare, they may find themselves at undesirable jobs; they may leave these jobs more quickly than more desirable jobs. The study of the workfare program in New York City in fact suggested very high welfare recidivism.[14] It is thus possible that workfare may hasten departures from welfare but also speed up returns to welfare in the intermediate or long run. Moreover, some clients leaving one welfare program may find other income transfer programs so that the net saving is smaller than that appearing by looking at the one program alone. Unfortunately, neither previous studies nor this one will observe clients over a long enough time to evaluate such long term possibilities.

D. The 100-Hour Rule

Since this study includes two experimental groups, one of which receives only the 30-and-1/3 treatment, it offers a chance to test the pure impact of changing the 100-hours limitation as well as of work experience. While the duration of this study may be too short to estimate the effects of a permanent change, as of now very little is known about the impact of this regulation which in its current form might be discouraging work. Some information, however, has been obtained in California, where between 1967 and 1971, the limitation on hours worked while in AFDC-UF was reduced from 173 hours per month to 100 hours per month. A study by Wiseman using longitudinal data suggests that these changes reduced the probability that men would leave the AFDC-UF program.[15]

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[14] Op. cit., Lieberman Research Suburban, p. 30

[15] Michael Wiseman, "Change and Turnover in a Welfare Population," in The Income Dynamics of the Poor, a report submitted to the U.S. Department of Labor, January 1977, p. 277.

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THE PROCESS SECTION

What was the Massachusetts Work Experience Program and how did it evolve? This section discusses these questions. It describes and analyzes the administrative processes and issues involved in making the program operational. The process section is basic to an understanding of the outcomes of this planned experiment.

CHAPTER III

A Welfare Experiment in a Turbulent Setting •

The basic tasks of this chapter are to describe the development of MASSWEP, its origins, design, and acceptance or rejection by key institutional actors in what became a rather turbulent setting for a modest welfare experiment. The original design for a Massachusetts workfare program is discussed and how the actual MASSWEP evolved is reported.

Data for this chapter came from several sources. The origins, design, and early stages of the program are described in a variety of DOL memoranda and internal MDES and MDPW memoranda made available to us by state officials. The implementation of MASSWEP was also followed closely by the media, advocacy groups, state legislators and by the Brandeis evaluation group. Structured interviews were conducted with 21 MDES/WIN and 20 MDPW/SAU staff representing all MDES/WIN and MDPW/SAU regions in Massachusetts. Circumstances dictated, however, that these interviews be held early in the 15-month MASSWEP program. When it was learned that temporary MDES/WIN staff employed through CETA for the MASSWEP program were to be dismissed as part of a general cut-back in CETA, Brandeis process analysts interviewed additional CETA workers to obtain their views of procedures used in the first months of MASSWEP. Finally, on a routine basis the Brandeis process analysts made extensive visits to MDPW and MDES/WIN offices to learn generally about the procedures followed throughout the program.

A. Origins of MASSWEP

1. The Massachusetts Economy and Welfare Caseload

In late 1976 and early 1977, then Governor Michael Dukakis became alarmed about the precipitous rise in the AFDC and AFDC-UF caseloads. High and rising welfare and Medicaid costs had forced him to propose major increases in taxes when he assumed office in 1975. Left uncontrolled, he feared continued increases in such costs would force further tax increases which might jeopardize the competitive position of the Massachusetts economy. Governor Dukakis thus pressed for analyses of the increases in caseloads and costs by particular welfare categories. Among other conclusions reached in these analyses was one stressing the need for various types of employment programs to return thousands of AFDC-UF fathers to work. One such effort, to include AFDC mothers as well, was work experience programs for those who were jobless for long periods. In the view of the Dukakis administration, a work experience program had two appealing features: first, having men on a work site could provide a quasi-market test of employability, something necessarily lacking in reappraisals conducted in employment offices; second, a work site experience could provide skills and work habits that would heighten the appeal of welfare clients to employers in both unsubsidized and subsidized jobs.

The economic climate in Massachusetts in late 1976 and early 1977 was improving in comparison to late 1975. The unemployment rate had declined from a high of 12.2 percent in May 1975 to 7.7 percent in December 1976. Largely because people were exhausting their unemployment insurance (UI) benefits, AFDC-UF caseloads still were rising in late 1976 and early 1977, hitting a peak of 6600 cases in March 1977. Between January 1976 and

March 1977, the AFDC-UF caseload had risen by 50 percent. Between January and February of 1977, the AFDC-UF caseload rose by 600 families or 11 percent, an astonishing jump for one month.[1]

In the context of an improving but still relatively weak labor market combined with a burgeoning welfare population, Governor Dukakis ordered special efforts to place AFDC-UF fathers in regular (unsubsidized) and CETA jobs. Studies were to be conducted on why those remaining on AFDC-UF could not be placed. The Governor's interest precipitated responses from Massachusetts agencies responsible for employment programs for low-income people. For example, a case-by-case analysis of the 600 family increase was conducted by MDPW and revealed that almost all of the increase was attributable to the exhaustion in two-parent families of UI benefits.[2] The MDPW used this finding to justify their seeking state legislation to enable the MDPW to recover AFDC-UF payments made while clients wait for their first, retroactive UI check.

Special efforts also were made by the WIN program. A special placement program was begun in Feburary 1977 and continued through August 1977 aimed at men in AFDC-UF. As a result, approximately 24 percent of the MDES/WIN caseload found jobs and 40 percent were referred by MDES/WIN to jobs in the first two months, while the monthly "entered employment" rate for

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1 Alexander Sharp, MDPW Commissioner, February Caseload Increases in AFDC-UF and AFDC (memorandum), March 31, 1977.

2 Ibid.

AFDC-UF clients in the remaidner of the period averaged 12 percent of the total AFDC-UF caseload.[3,4] During this period, the AFDC-UF caseload dropped by 21 percent.[5]

2. Political Responses to the Initial Workfare Plan

In December 1976 the Governor submitted an internal memorandum to his Secretary of Economic Affairs outlining a workfare plan for all 60,000 WIN registrants on AFDC and AFDC-UF. In his plan, clients would work up to 52 weeks without pay. As conceived by the Governor, workfare was to be a separate program, implemented outside the structure of the WIN program. The Governor's workfare proposal was in sharp contrast to the recommendation of a special Task Force on Job Creation which he had established. The purpose of the Task Force was to recommend job programs for welfare recipients. The panel concluded that a mandatory work program was unworkable.

As rumors circulated, the Dukakis plan attracted immediate and widespread opposition. Within the Dukakis administration, the Secretaries of Human Services and Economic Affairs wrote memoranda arguing against the program, principally because of its cost. Workfare programs in other states were reviewed and evidence from these experiences were accumulated by staff in the Human Services and Economic Affairs departments. In addition, opposition began to form in the legislature and among advocacy groups.

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3 John A. Murphy, MDES Director, AFDC-UF Special Placement Project (memorandum), April 8, 1977.

4 MDES, WIN AFDC/UF Monthly Reports, May 31, 1977-August 31, 1977.

5 MDES, Summary of Statewide AFDC Placement Activity, March 1, 1977-August 31, 1977, prepared for September 30, 1977 meeting at University of Lowell, Lowell, Massachusetts.

In response to institutional pressures and opposition, the Massachusetts proposal for a workfare program underwent many changes. On February 25, 1977, the Governor inquired of the Secretary of HEW whether a waiver of the rule restricting WIN work experience to 13 weeks could be obtained to test an alternative workfare project now directed at AFDC-UF fathers. Concurrently, numerous advocacy groups coalesced into an "Ad Hoc Massachusetts Coalition Against Workfare" (MCAW). MCAW notified the acting director of the regional HEW office of its opposition to the Governor's request for a waiver of WIN rules. Continuing their opposition, more than 100 members of MCAW met with the HEW representatives in May 1977, attracting considerable attention from the media. This publicity brought previously unaffiliated church representatives, social work organizations and human services associations to the Coalition. The Governor's continuing support for some kind of mandatory work program was symbolized by the appointment of a full-time coordinator, the Assistant Economic Affairs Secretary, to oversee the development of alternative proposals. In June 1977 the Dukakis administration presented a modified plan called the Work Experience Program, a 52-week program for unemployed fathers in the WIN program. In response to the June proposal, 500 people attended a public hearing to voice opposition to the plan, and MCAW reinforced this opposition with demonstrations across the state. The new plan would provide "recent" work experience for "long-term" unemployed men on AFDC-UF rolls. In September 1977, to implement the program, the workfare coordinator sought an amendment to the State Title IV-A plan rather than the previously requested more complex waiver of WIN rules.[6]

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[6] Richard Anderson, Assistant Secretary of Economic Affairs, Status of Work Experience Program (memorandum), September 29, 1977.

About the time that Governor Dukakis released his new format for a work experience program (June), he began to receive at least tacit support from the agencies administering WIN--MDPW and MDES. These agencies were anxious to strengthen the work experience component of WIN, and perhaps even adopt the Governor's scaled-down proposal within their agencies. Studies based on a review of MDES/AFDC-UF files supported the Governor's modified proposal by identifying that "the greatest single barrier to finding jobs for many welfare fathers is a lack of recent job history."[7] While over 80 percent of the fathers had held full-time jobs, one-third of the caseload had been jobless for three or more years. An MDES report summarized the analyses by stating:

> While many referrals of AFDC-UF fathers are still pending, approximately 25 percent to 30 percent of the 5600 fathers have not been hired. Among this group, a large number, according to interviews with WIN caseworkers, were not hired as a result of insufficient experience. In the month of May (1977), despite an expansion of CETA funded public service employment jobs, 28 percent of unemployed fathers referred to jobs were not hired. These men are concentrated in the unassigned recipient pool. They once had salable skills, but prospective employers are frequently reluctant to hire these men after such a long hiatus from the labor force, despite inducements such as tax credits offered by the state and federal governments to employers who hire WIN registrants. As a result of their prolonged unemployment, many of these men have lost good work habits and suffer from low esteem."[8]

Modifications of WEP proposed by Governor Dukakis in June 1977 did not mollify the opposition. Protests continued to mount, and in early October, representatives from the Coalition as well as several state legislators met with HEW staff in Washington to consider criticisms of the

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7 Harold Doherty, Update of Characteristics of UF Cases, July-December, 1976 (memorandum), MDPW, April 27, 1977.

8 MDES, The Work Experience Program (WEP), Publication #9842-5-3000, July 1977.

program. HEW representatives revealed at the meeting that the Dukakis administration would not be allowed to conduct a special work program outside the WIN regulations. The Coalition then directed its mounting energy to influencing the U. S. Department of Labor.

In early November 1977, representatives of MCAW met with the Secretary of Labor, Ray Marshall, who agreed to review the objections of MCAW to workfare. The following day the position voiced by MCAW acquired new strength when the Massachusetts State Labor Council at its 20th Convention passed a resolution against workfare. On November 18, the MCAW Coalition further buttressed its position by releasing the names of 100 social service agencies across the state who refused to participate as sponsors of work sites in the workfare program. The DOL responded to an inquiry from Massachusetts on the merits of a long-term work experience program and concluded that even a 26-week program was legally incompatible with WIN regulations and the Fair Labor Standards Act; the latter limited work experience programs to 13 weeks, with a possible second 13-week period for individual cases.[9]

Faced with these pressures, Massachusetts officials in the executive branch decided to develop their plan within the WIN program, thereby assuring enrollees of necessary supportive services as a condition of enrollment as well as the protection of the sanctions process included in WIN. Massachusetts officials had first contemplated 52 weeks of work experience, then 26 weeks, but within the WIN framework had to settle for 13 weeks. It was, however, agreed to allow for the possibility of an additional 13 weeks following a second reappraisal if it was concluded that a second period of

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9 Alfred Albert, Deputy Solicitor, USDOL, Memorandum to Ernest Green, Assistant Secretary, USDOL, Massachusetts Workfare Proposal, November 29, 1977.

work experience was "necessary to equip the individual for entry into the occupation he seeks or to qualify him for a different occupation."[10]

Utlimately, the Secretary of Labor approved a Massachusetts plan for a 13-week work experience program, within the existing WIN program, but not without first offering the Governor an alternative demonstration, similar to the Minnesota Work Equity Program, costing about five million dollars and paying minimum wages, which could partially offset welfare benefits. The state rejected this alternative.

B. The Final Design of MASSWEP

The final first year plan provided $583,333 in special funds to Massachusetts. The aim was to serve 2000 unemployed fathers thought to be employable but lacking in recent work experience. The work experience component within the WIN program would be a central feature of the new MASSWEP program. While it had been offered to women on a voluntary basis, it had rarely been used for men. In MASSWEP for men found suitable, it was to be required as a condition for continued benefits. In addition, HEW representatives and the Governor's workfare coordinator had injected a test of modifying the 100-hour rule. They were particularly interested in studying whether removal of the disincentive alleged to stem from this rule would move men to get jobs. The details of the MASSWEP treatments have already been described in Chapter I. This section will thus concentrate on the management structure of the program.

The U. S. Regional WIN Coordinating Committee, representing USDOL and USDHEW, approved the MASSWEP experiment in December 1977. The Director of MDES and the Commissioner of MDPW jointly accepted responsibility for

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10 Ibid.

MASSWEP, but the responsibility for managing the program was lodged with the state WIN program. This, in turn, meant that the MDES/WIN and MDPW/WIN directors shared responsibility for the program.

Basically, the administrative structure for MASSWEP, since it was part of the WIN program, follows the structure for WIN, with one special addition--the WEP specialist, to be discussed below.[11] There are six MDES and six MDPW regions in Massachusetts. The WIN program is part of this regional system and has six regional coordinators on both the MDES and MDPW sides. At the local level there is a WIN team consisting of an MDES worker and an MDPW-SAU worker.

Of special interest in MASSWEP are "WEP Specialists" who play a staff role. These persons were to be hired through CETA to assist the various WIN Regional Coordinators in managing MASSWEP. The specialists were to interact regularly with WIN teams in local offices as well as with MDES and MDPW Regional Directors. Specifically, the specialists were to be responsible for establishing and maintaining an elaborate management information system. This system was to be a major tool by which the state was supposed to monitor MASSWEP, and serve as well some purposes of the Brandeis evaluation, although these specialists were eliminated after the start of the program.

In all, the final design of MASSWEP authorized 65 people working for MASSWEP--two hired through CETA for each of the six MDPW regions, one from CETA for each of the 37 MDES/WIN teams and one for each of the six regional MDES offices; and ten additional MDPW/SAU workers, not hired through CETA, to carry out the expanded assessment procedures required in MASSWEP.

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[11] For a detailed institutional analysis of WIN, see Mitchell, Chadwin, and Nightingale, Implementing Welfare-Employment Programs: An Institutional Analysis of the Work Incentive Program, Urban Institute, October 1979.

Since there were many delays in the start of the experiment, many of the temporary employees were already scheduled to be terminated when the program was finally underway in January 1978. Both WIN coordinators and WEP specialists, whose attention should have been devoted to MASSWEP itself, were forced instead to devote time to trying to extend the appointments of the WEP specialists. The problem grew until the entire group of WEP specialists, and the data system they were to manage, were abandoned in the summer of 1978.

The problem of staffing a temporary program with temporary workers was complicated by a parallel problem involving the understaffing of MDPW/WIN with SAU workers. The chief supervisor of the central MDES/WIN office anticipated difficulties with this aspect of the program before MASSWEP even began and felt that the lack of SAU staff would be a major problem in the project. While MDPW had requested--and received--federal funds for additional SAU workers, not all the allocated positions were filled 12 months after MASSWEP began. The MDES/WIN supervisor noted that because of the absence and shortage of SAU workers, seven out of the 35 offices would not be able to comply fully with MASSWEP rules.

In addition to the staffing problems there were coordination problems. MASSWEP depended on the cooperation of two state agencies, but with inadequate mechanisms to secure and guarantee inter-agency cooperation for MASSWEP. Customarily, within the WIN program, counterparts within MDES and MDPW are supposed to be in routine communication with each other. Only at the front line, however, where WIN teams see clients, did the MASSWEP program formally mandate interaction in the joint MDPW and MDES employability assessments of AFDC-UF fathers.

C. Reactions to the Final Design of MASSWEP

1. Official Reactions

After 18 months of planning and seeking federal authority, the Governor of Massachusetts was able to start a program, which, if not the workfare program originally envisioned, at least had many similarities to it. "Employable" welfare fathers who met formal program criteria would be required to work 13 weeks without pay in the work experience component of WIN and with penalties for non-compliance. The Governor held that a good program had been adopted that incorporated the essence of the earliest plans with noticeable improvements by virtue of the public debate and negotiation process. It could even be argued that MASSWEP infused the Massachusetts WIN program with special financial resources to build up a WIN component, work experience, virtually unused heretofore in the AFDC-UF program for men. The clout of mandatory work assignments, and the potential use of sanctions, were welcomed on an experimental basis, and were expected to be useful in giving local WIN team personnel a new tool. In addition, the final design included the modification of the 100-hour rule for fathers, which some Massachusetts WIN officials thought would have a major effect.

WIN officials were less sanguine about the 13-week maximum work experience, even with the possibility of a second 13-week extension. One senior official noted flatly in December 1977 before MASSWEP began that the 13 weeks would probably help determine some clients' employability, but would be too short a time for clients to build up a work history acceptable to employers. He felt that the 13 weeks would not be long enough to adequately assess employability in most cases. The same official noted a further trade-off in a 13-week program--difficulty in developing work sites and hence a longer start-up phase for the overall effort.

The national WIN office also had a strong interest in MASSWEP. There was a desire to learn from MASSWEP about two issues in addition to work experience--first about the reappraisal process, and second, about the modification in the 100-hour rule for men. Both issues are important in a variety of income transfer plans. Most work registration requirements, for example, require screening and discretionary decisions by counselors on the suitability of clients for work. As for the 100-hour rule, it could discourage a significant amount of work, but empirical evidence on this is lacking. Finally, the national WIN office had been concerned about legal problems in the Governor's initial plan and was relieved to see MASSWEP which fit into the WIN program. The Governor's initial plan was modeled after a plan in operation in Utah that federal officials feared violated federal law. The controversy in Utah, and potentially with Massachusetts had the early plan been accepted, centered on the requirement in the Fair Labor Standards Act of a "fair day's wages for a fair day's work" and on the meaning of "acceptable work projects." In any case, federal WIN officials were relieved to avoid these legal battles in Massachusetts.

2. General Reactions Among Advocacy Groups

Predictably, the opposition to MASSWEP did not end with approval of the final design, although a MCAW coalition spokesperson called it a victory for groups opposing workfare and a "hollow shell that will serve as a political face-saving device for the Governor."[12] Using the federal approval of MASSWEP to build a grassroots organization based on broader welfare issues, MCAW sought to spread opposition to the new program, and to delay its implementation.

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12 Boston Globe, December 20, 1977, p. 3.

MCAW and unaffiliated workfare opponents, such as the League of Women Voters, National Association of Social Workers, and some state legislators, opposed the final form of MASSWEP for the following reasons:

- Since no wages were paid beyond the $30.00 monthly incentive and work related expense, the program violated the principle of "equal pay for equal work."
- The program lacked a training component and therefore was unlikely to improve the fathers' competitive standing in the labor market.
- The mandatory nature of the welfare jobs program was cited as a violation of basic liberties.
- Because the program would have difficulty in creating jobs for recipients, it was likely to force displacement of employed workers. The job development aspect alone was expected to require sizeable outlays and a costly bureaucracy.
- It was feared that work requirements would "frighten people off the welfare rolls" and make welfare a degrading experience.

These views were reported widely in the press during the early stages of the implementation of MASSWEP, but did not delay the program. As of January 18, 1978, all MASSWEP staff in MDES/WIN intensified the process of reviewing their caseloads. Concurrent with this review and "paper selection," MASSWEP staff followed up with solicitations among non-profit agencies for potential sponsors of work site agreements.

3. The Court Case and Injunction

In the first week of March 1978, the MCAW Coalition, with the assistance of the Massachusetts Law Reform Institute, brought MDES to court, alleging that the state had not followed strictly the Administrative Procedures Act (APA) in promulgating its regulations. MCAW had found a test case in the person of Leo Nault, an unemployed father. The APA requires that an agency publish a notice of proposed regulations at least 21 days

before they go into effect in order to provide interested persons with an opportunity for comment. Mr. Nault's complaint sought declaratory relief with respect to the validity of the regulations, and further requested that the MASSWEP Program be enjoined until the requirements of the APA were met.[13]

On March 2, Judge Paul Garrity of Norfolk Superior Court, granted a preliminary injunction until the defendant, MDES, complied with the APA. Four days later, the order of March 2 was modified to allow MDES to issue emergency regulations on the MASSWEP program. The following day, March 7, MDES promulgated emergency regulations.

On March 8, the plaintiff filed a motion for leave to file a supplementary complaint, with the complaint attached.[14] The following day the Court allowed the plaintiff to file his supplemental complaint challenging the validity of the agency's finding of emergency, the basis for the promulgation of emergency regulations. The Court also heard arguments on the plaintiff's application for injunctive relief, in which he once again sought an order prohibiting MDES from implementing the Work Experience Component in WIN until MDES complied with the provisions of the APA for advance notice and prior opportunity for comment.

On March 14, 1978, the plaintiff's application for a temporary restraining order and/or any preliminary injunctive relief was denied.

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13 Leon Nault's Complaint Against the Division of Employment Security, February 23, 1978. See also Affadavit of John Hodgman, Director, MDES, February 27, 1978; Findings, Reliefs, and Orders of Judge Paul Garrity, Norfolk Superior Court, Commonwealth of Massachusetts, March 3, 1978.

14 Memorandum in Support of Relief Requested in Supplemental Complaint, filed March 9, 1978 by Nault et al.; Plaintiffs vs. Hodgman et al., Defendants. See also Defendant's Motion to Modify Order March 7, 1978.

At the same time, MDES was ordered not to require those persons who did not wish to do so to participate in the Work Experience Program from March 14 through April 10.[15] During this period the mandatory Work Experience Program became voluntary for all.

Judge Garrity further ordered MDES not to continue to implement the Work Experience Program except for those individuals already participating in the program who wished to continue to do so, until MDES complied with the advance notice, prior opportunity for comment, public hearing, filing, and publication requirements of the APA. Of the 66 individuals then enrolled in MASSWEP, 42 signed waivers and chose to stay and complete their assignments.

MDES held the mandated public hearing on April 18, 1978, as required by the Court Order. Approximately 250 opponents of workfare attended the hearing; spokespersons from over a dozen organizations testified against the program. The Director of MDES, the MDES legal counsel, and WIN director sat quiety through the hearing, thus fulfilling the court's order without responding to specific objections raised by the testimonies. The final MDES regulations for MASSWEP were published in accordance with the APA on April 17, 1978. On May 15, 1978, the Director of the MDES/WIN program instructed WIN teams to reinstitute MASSWEP, just as it had been before the injunction.

The injunction period had delayed the implementation of a program that would have encountered "normal" start-up problems even without the

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15 Order and Memorandum, March 14, 1978, Judge Paul Garrity, Norfolk Superior Court, Commonwealth of Massachusetts, March 14, 1978.

added burden of a court case.[16] Although the legal battle was directed toward procedural issues, and left MASSWEP intact, it did heighten sensitivities among program operators to the importance of following the formal program rules. One result was intensified training sessions that stressed "quality" paper selection, reappraisal, referrals to alternative WIN components as well as work site referrals. This was in contrast to earlier goals of moving a large number of persons through the system. The fear of another legal battle meant that MASSWEP proceeded more cautiously when it began again in May 1978.

After May 1978, MASSWEP proceeded with practically no external pressures. The Coalition held its last public rally in April 1978; the Governor's "workfare coordinator" left office; new court cases which had been discussed by MCAW failed to develop; press coverage became infrequent; and a new Governor was elected in the Commonwealth. The one exception

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16 The experience of MASSWEP was similar to that of other mandatory programs. These programs frequently invite legal challenges. Legal resistance usually serves to slow rather than stop the projects. See, for example, legal cases in California, Kentucky, Michigan, Minnesota, New Jersey, and New York.

California: Community Work Experience Program--Third Year and Final Report, State of California Department of Employment Development, DHEW Project, April 1976.

Kentucky: David Lollis, Bureau of Manpower Services, WIN, State of Kentucky, Personal Communication.

Michigan: A Study of the General Assistance Workfare Programs, National Association of Counties Research, Inc., February 17, 1978.

Minnesota: Minnesota Work Equity Project--First Interim Report, Abt Associates, Cambridge, Massachusetts, March 1979.

New Jersey: New York Times, November 9, 1978, "Bordentown's (New Jersey) Workfare Cuts Welfare to the Bone."

New York: op. cit., (NACO).

to the "peace" were attempts by MCAW to obtain names of organizations accepting MASSWEP fathers, as well as organizations considering participation in MASSWEP, to discourage their participation. These strategies had very limited effect on the operations of MASSWEP after May 1978. Consider, for example, that only one client reported on the Brandeis survey of men who had attended work sites that "a group opposed to WEP" pressured him in some manner. Similarly, only three work sponsors of 60 interviewed reported that a group opposed to workfare had contacted and pressured them in some manner. Finally, of 215 fathers who responded to a follow-up survey regarding reasons they did not attend work sites, only eight cited as reasons that persons or groups opposed to MASSWEP encouraged them not to participate.

The political scene regarding MASSWEP indeed was quiet in the post-injunction phase. By the time of the election of Governor Edward J. King in November 1978, MCAW had disbanded to form new alliances around other emerging welfare issues. The welfare agenda of the new Governor did not contain a single reference to MASSWEP, nor have inquiries been made of the Brandeis researchers since Governor King's election as to the success of the program. The current administration instead has focused on cost reductions relating in large part to welfare eligibility; fraud and overpayment errors have been major concerns. A related concern in the King administration is the overall effectiveness of the AFDC-UF program, as well as CETA/WIN relationships. Although these concerns touch some of the broad concerns of MASSWEP, it can be said that since May 1978, except for the lingering effects of earlier interest, the MASSWEP program was left unencumbered by political pressures.

D. Conclusion

While first conceived in the midst of a welfare crisis in late 1976, mandatory work experience for AFDC-UF fathers eventually was implemented in early 1978 in a very different context. A steadily improving state economy coupled with a special placement effort in the WIN program that was finding jobs for AFDC-UF fathers at a monthly rate of 12 to 16 percent contributed to a sharp fall in the AFDC-UF caseload during most of 1977. Although this trend was reversed by the time of the inception of MASSWEP in early 1978, a welfare crisis did not exist at the time of its implementation.

Conceived, developed, and implemented in a politically turbulent setting, the original workfare plan underwent many changes before it became MASSWEP. Actors and interest groups at the various levels of government perceived this pilot program as a possible forerunner of a nationwide workfare program and fought hard over its details. Not surprisingly, the program was changed substantially from the time it was initially proposed. The original "workfare" proposal covered all WIN registrants, not just AFDC-UF fathers; made workfare a condition of eligibility for benefits, and was not simply one component of a client's employability plan developed on a client-by-client basis; lasted for up to 52 weeks for each client, not 13 weeks; was to result from simple MDES interviews, not from more complex WIN reappraisals done jointly by MDES/WIN and MDPW/SAU staff; contained a streamlined sanctions procedure as opposed to the more involved one in the WIN adjudication system; and was to be administered by CETA staff on WIN teams, persons who were not part of the regular WIN program. Ultimately, however, MASSWEP became a program within a program--a part of WIN.

Even once initiated, MASSWEP did not run smoothly. There were numerous difficulties in staffing and coordination whose consequences will be observed

in the next chapter. In addition there were legal problems, culminating in the injunction. Such difficulties are, of course, by no means unique to MASSWEP. They are of interest, however, for two reasons. First, it must be expected that new mandatory work programs will face major start-up difficulties. Unfortunately, our evidence is not sufficient to judge how long this start-up phase can be expected to last. Second, because of these difficulties, caution is needed in evaluating the impacts of the program. It is unfortunate that most experimental programs must be evaluated during a phase when they are not yet operating efficiently.

CHAPTER IV

Administrative Processes

This chapter investigates the process of administering the MASSWEP program. The process analysis of MASSWEP addresses a series of issues. First, the public is interested to some degree in whether those managing the MASSWEP program performed well by standards related to process: were recipients paper selected in the proper proportions; were the selected persons reappraised; were sufficient numbers judged suitable for assignment to workfare; did those who were assigned appear at and continue to report to their work sites; and, for those resisting the work site assignment, were sanctions applied? Secondly, policymakers need to understand the causes of deviations from plans, so that the feasibility of workfare can be assessed and that replicas of workfare can be appropriately adjusted. Lastly, to interpret the results of the ensuing impact analysis, it is necessary to know what actually happened, especially if this diverges from what was planned. The process analysis prepares the background for the impact analysis.

The discussion in this chapter is organized around the major stages of the program--paper selection, reappraisal, and experimental treatment. At each stage rules of the program are characterized, actual procedures are described and analyzed; and the relationship between rules and actual procedures is investigated. The chapter

concludes by drawing lessons from the process analysis regarding the design, replication and potential impact of a workfare program like MASSWEP.

Some of the technical issues in accounting for the numbers of clients passing in and out of each stage of the process are discussed in Appendix D. It was decided to put this discussion in an appendix, first because the quantitative data is often incomplete so our arguments are somewhat speculative, and second, partly as a result, our arguments and inferences are sufficiently complicated that the average reader might not want to take the trouble to read them. But they are available to all who so desire in Appendix D. This chapter introduces the relevant factors that mattered in the process; it is just that their quantitative impacts are not fully discussed.

A. Paper Selection

MASSWEP was to begin with a "paper selection" procedure, that is, a preliminary screening of files containing clients' folders to identify those who seemed appropriate for WEP. Those selected would then be called in for a personal reappraisal interview, the next step in the MASSWEP process. The paper selection was to be performed by MDES/WIN personnel in each of the 35 WIN offices in the state. Since the paper selection was an internal office procedure, it seems unlikely that any individual client would know that his folder was

receiving this perusal. Individual clients were no doubt aware of the MASSWEP program through media publicity, but its personal impact on them could not be known until after they were paper selected and then only if they were called in for a reappraisal interview or other WIN treatment. We consider first the rules for the paper selection and then procedures actually followed.

1. Rules for the Paper Selection

The rules for the paper selection were apparently simple, but there was a slight change in wording at the time of the injunction which could have been significant. It is therefore worth considering the rules in detail. The rules for MASSWEP were specified in the Amendment to the FY '78 Massachusetts WIN Plan (referred to as WIN Amendments), submitted jointly by MDES and MDPW on December 15, 1977. With respect to the paper selection, these rules state:

> The DES/WIN teams will identify in their files Unemployed Fathers in the unassigned recipient category who have been unemployed for at least six months and registered with the WIN team for at least three months. Those fathers unemployed for the longest period will be called in first. These are participants who have been referred unsuccessfully to subsidized or unsubsidized employment and who have been found unsuitable for referral to other WIN components. They will be called as potential participants for work experience.

These rules were circulated to Regional Directors and WIN coordinators attached to a memo dated January 3, 1978. Procedures for Implementing the FY '78 AFDC-UF Work Experience Initiative As Part of the Massachusetts WIN Program (referred to as Implementation Procedures) were circulated on January 12, 1978 as an attachment to L.O.B. No. 82-N. This document states:

> Upon receipt of this bulletin, all WIN teams will review AFDC-UF cases presently in "unassigned recipient" WIN status and identify those registrants for whom assignment to the work experience component seems appropriate.

Presumably those who "seem appropriate" are those who satisfy the more specific criteria of the WIN Amendments. Following the court injunction, MDES issued the WIN Work Experience Initiative Regulations, to become effective on April 11, 1978, and to supersede previous regulations. These rules state:

> All WIN teams will review AFDC-UF cases presently in "unassigned recipient" WIN status and identify those registrants for whom assignment to the work experience component seems appropriate. Any unemployed father, in order to be considered for this initiative, shall meet the following criteria:
>
> a. Unemployed for a minimum of six months, and;
> b. Registered with the WIN program for a minimum of three months, and;
> c. Unsuccessfully referred to subsidized or unsubsidized employment, and;
> d. Is unsuitable for referral to other WIN components.

The terms used in these revised regulations are identical to those used previously, but their order suggests a different interpretation. The four criteria stated here are identical to the conditions appearing in the WIN Amendments. But here the term "seems appropriate" appears first and does not seem to be defined by the four criteria. Instead, "seems appropriate" can be interpreted as an additional criterion: the four specified conditions must hold and in addition the client must seem appropriate. The significance of this interpretation is that it allows a measure of discretion to anyone performing a paper

selection. The first three criteria can be objectively applied. The fourth criterion--suitability for other WIN components--could introduce an additional element of discretion. But "seems appropriate" as a separate criterion is potentially the more important of the two discretionary elements. It may be quite a reasonable criterion if the WIN worker knows of special circumstances that could make the client inappropriate. But in making such a determination, the WIN worker has a degree of choice that could lead to uneven treatments of clients. Indeed, a considerable variation was observed among regions in the implementation of the paper selection. One contributing factor could be the leeway permitted to WIN workers by these rules.

2. Actual Treatment Under the Paper Selection

Two broad questions need to be considered when examining the actual paper selection. First, were the criteria applied correctly--without error--in selecting or not selecting clients for further reappraisal? This question leads to an examination of the administration and procedures of the paper selection. Second, how many people were selected and not selected and what were the actual reasons for not being selected? This question contributes to an evaluation of the need for the program, for if few people are or should be paper selected, this need must be small. At the same time, reasons for not being paper selected could provide useful information on needs of the current WIN program. While the second or numbers question depends on

the first, we will deal with them separately. The administration and procedures will be discussed in this chapter while some of the technicalities of the numbers question are reserved for Appendix D.

The central question relating to administration is whether clients were treated correctly in terms of the rules. A high degree of variability was found in the proportions paper selected from one region to another. For example, for the period from February through June of 1978, Table IV-1 shows the wide disparities

Table IV-1

Regional Variation in Proportions Paper Selected[a]
(January through June 1978)

Region	Number in Unassigned Recipient Pool as of January 1978	Paper Selected (Number)	Paper Selected (Percent of Unassigned Recipient Pool)
1	618	274	44
2	1251	195	15
3	578	122	21
4	662	107	16
5	772	181	23
6	1106	675	61
Total	4987	1554	31

a/ Source: MDES, "State WIN worksheets"; and MDES Progress Report on Massachusetts Work Experience Program, to USDOL, August 24, 1978.

in the rates at which men were paper selected out of the unassigned recipient pool.[1]

The rates range from a low of 15 percent to a high of 61 percent. Some of this variation might reflect errors in treatment, the exercise of discretion by WIN staff, or larger concentrations of appropriate clients in some places than in others. Unfortunately, our data sources do not permit a full accounting for all the variability. However, we do have a limited amount of information on the extent of error. In addition, we shall look at a number of special circumstances in the history of the program as well as some of the organizational background of the paper selection to obtain insights on the sources of the observed variations.

a. Errors in Treatment Under the Paper Selection

Although the research staff did not attempt to measure the extent of error, a high level MDES/WIN administrator undertook just such an effort part way through the program as a quality control mechanism. He reviewed a sample of client files at ten of the thirty-five local offices, although he was evaluating both the paper selection and the reappraisal simultaneously. He concluded that ten to thirteen

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1 We did not gather data independently on the paper selection, but relied on state sources which were not available for periods other than that reported on here. Notice in particular that the rate of paper selection depends on both the number paper selected and the size of the unassigned recipient pool within which the paper selections were made. The only time when reported figures on the unassigned recipient pool could be used was the beginning of the program. After that point, the reported figures on the unassigned recipient pool would include some who had been through the paper selection, but not selected. On the other hand, the initial unassigned recipient pool would be suitable for calculating the rate of paper selection only in the initial phase of the program, since new clients would be added to the pool with the passage of time.

percent of the files were selected (and reappraised) erroneously.[2] His review identified individual clients whom he thought were treated erroneously to enable the WIN staff to take remedial actions. Unfortunately for our purposes, we obtained from him information only on the overall rate of error--on whether people were treated incorrectly on the basis of the whole set of criteria for the paper selection.

Some independent information from Brandeis data sources[3] shed light on errors in applying individual criteria for paper selection. "Barrier forms" were completed by the MDES/WIN staff for Brandeis for those clients who reached the reappraisal stage. Data from these forms indicated three percent of these clients were unemployed full-time for less than six months prior to their reappraisal; therefore they were certainly unemployed full-time for less than six months prior to their paper selection which preceded the reappraisal. (Re-appraised cases constituted 55 percent of those paper selected.) Brandeis on its own interviewed a sample of those who had been assigned to experimental sites and who had thus necessarily passed the paper selection. Over half of those interviewed could not recall being referred to a CETA job prior to reappraisal, another criterion for paper selection. Of course some of these may have applied for CETA jobs on their own or suffered from recall problems. It seems quite

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[2] Some of the errors reflected no more than a failure to revise Employability Plans after reappraisal.

[3] A complete list of all data sources used appears in Appendix A .

possible that many clients were never referred to CETA--an error.

b. Special Circumstances in the History of MASSWEP Relating to Paper Selection Prior to the Injunction

(1) Pre-program preparations for paper selection

The rate of paper selection during a time interval is the number paper selected divided by the size of the unassigned recipient pool within which the paper selections were made. The rate is thus affected by changes in either numerator or denominator. Further, it is obvious that the rate depends not only on the size but also on the composition of the unassigned recipient pool: the more individuals in the pool who happen to satisfy the criteria, the higher the rate will be. Developments shortly before the beginning of the program had a significant impact on the rate because they changed both the size and composition of the unassigned recipient pool.

While MASSWEP started officially on January 12, 1978, activities in the WIN program in preceding months were affecting its clientele and thus, probably, its overall effects. Attention has been given already to a placement effort in behalf of AFDC-UF fathers, launched in March 1977. Along with an improving economy, this effort probably helped reduce the AFDC-UF rolls from 6600 cases in March to 5200 cases in August. However, the AFDC-UF caseload was back up to 6000 in January 1978 (and 6150 one month later). This placement effort may have had complex effects on the subsequent paper selection rate. It may have removed the less difficult cases, leaving a higher representation of long-term unemployed in the unassigned recipient pool and

thus a paper selection rate. On the other hand, it is conceivable that some fathers were placed in only short-term jobs during the spring and summer, returning to AFDC-UF and then to the WIN unassigned recipient pool in late 1977, as short-term unemployed; if so, this effect could have had the opposite effect on the rate.

Along with the placement effort during 1977, there was a review of AFDC-UF cases in the WIN files. This review was designed to identify WIN registrants who should have been deregistered, especially those who no longer were receiving AFDC-UF payments. Probably more than anything else, this review reduced the unassigned recipient pool from 7100 in June 1977 to 5200 in June 1978.

The placement effort of 1977 and the review of cases meant that an important step in MASSWEP paper selection, was in a sense underway in some offices before MASSWEP formally began. The review, conducted more industriously in some WIN offices than in others, meant that some local offices were more prepared than others for the paper selection process in MASSWEP. In fact, two (of the 35) local WIN teams actually had completed paper selection, reappraisal and assignment after MASSWEP was only six weeks old. Forty percent of the 3120 persons who ever passed paper selection did so by the end of February 1978.[4] Variations between offices in the extent of pre-program may have been an important source of the differences in paper selection rates.

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[4] Massachusetts Division of Employment Security, AFDC-UF Work Experience Program Management Information Summary, 2-1-78 to 3-22-78.

(2) The court injunction and paper selection

However well paper selection conformed to prescribed procedures in the early stages of the program, changes in several aspects of MASSWEP, including paper selection, resulted from the court injunction (described in Chapter III) that was issued on March 14, 1978, two months after the program began. MASSWEP was converted into a voluntary program for all AFDC-UF fathers during the injunction, even for those on worksites, and was generally in a "hold" position until May 15, 1978, when the injunction was lifted.

Following the lifting of the injunction, the program was renewed but officials at every level seemed to become more cautious in their implementation of the various features of MASSWEP. In the discussion of rules we have already seen the apparent elevation of the "seems appropriate" clause to a separate criterion. In terms of actual procedures, the most noticeable change was that WIN staff became more attentive to the criterion that men be referred to CETA jobs before they were paper selected.

(3) Later developments in paper selection

In late 1978 and early 1979, state WIN administrators tried to revitalize MASSWEP. Revised objectives were announced. These were more conservative than those declared about a year earlier. At training sessions for local WIN office personnel, the estimate of the original unassigned recipient pool was revised downwards; in

paper selecting from the unassigned recipient pool, staff persons now were instructed to consider carefully alternative WIN strategies, and, in general, the emphasis turned to selecting fewer but more likely prospects for assignment to work sites. It follows that expectations for paper selection had been lowered. While in the early stages of the program, the paper selection may have been used to place as many as possible in MASSWEP, by the later stages of the program WIN workers were encouraged to use the fourth criterion for paper selection, availability of an alternative WIN component, whenever possible. At one of these late training sessions, a high level WIN administrator said: "We don't want [them] in [MASSWEP] if there is a better strategy."

c. Organizational Background of the Paper Selection

A closer look at organizational behavior reveals additional pressures affecting the paper selection rate differentially across offices. This involves looking at the motivations of the various types of WIN workers as well as at features of the environment and formal structures within which they operate.

First, some WIN staff tried for one reason or another to make MASSWEP succeed, while others were indifferent or opposed. Of particular interest were the persons hired from the CETA program who joined with regular WIN staff in conducting the paper selection. Those hired from the CETA program noted to our process analyst that they could accept MASSWEP more easily than could the regular staff;

they did not share the protective attitudes of the latter toward WIN clients. The CETA workers also were more responsive to the requests of state WIN officials for rapid action resulting in assignments to work sites because such personnel wanted to establish credentials for permanent employment with WIN. (At least six of the 40 persons hired through CETA expressed bitter disappointment eventually when they were not hired.) Some of these WIN workers told us that they tried harder than others to include AFDC-UF fathers at the stage of paper selection.

Second, as minor as it may seem, differences in filing systems across and within the offices may have noticeably affected productivity. Our process analyst concluded that where files were kept in drawers that were accessible generally, aggressive selectors were able to conduct paper selections with ease. But in some offices, individual WIN staff kept the files of clients assigned to them on their own desks. So small a difference could noticeably complicate the tasks of the staff assigned to perform the paper selection. Another complicating factor relating to files occurred in those offices where WIN/AFDC-UF records were mixed with WIN/AFDC records.

Third, there were structural differences between regions in the way the paper selection was organized. In one region WIN and SAU personnel, including MDES and MDPW regional coordinators and specialists, collectively made paper selections in group meetings. Elsewhere, the procedure was executed by MDES/WIN workers.

Further, there were differences in the interpretation of the "seems appropriate" criterion. There were cases of clients not paper selected because of medical, alcoholic, language and age-related problems, even though they qualified on the other four criteria. At the other extreme, highly employable sorts with generally good work histories, or high motivation, or very good prospects for early re-employment also were excluded. Some of these may have received placement assistance from WIN, but we do not have information on the actual disposition of their cases. While the magnitude of these exclusions cannot be gauged, it can be said that the majority of the 35 WIN offices admitted to excluding three to 15 men for reasons related to a local judgment about employability. Typically, these were cases in which WIN staff, exercising allowable discretion, excluded AFDC-UF fathers because the men were judged to be either hopelessly unemployable or highly marketable.

B. Reappraisal

While the paper selection was a preliminary screening of the unassigned recipient pool based on office files, the reappraisal made the final determination of appropriateness for work sites. Only those who had been paper selected were to be called in for a reappraisal interview. Both MDES/WIN staff and an SAU worker were to participate in the reappraisal, either jointly or sequentially. The reappraisal was to evaluate why the client had not found employment. The outcome of the reappraisal would be identifying the best treatment for the client, one

option being assignment to work experience. The list of client problems to consider was specified as follows:[5]

a. whether there is any physical or emotional problem which warrants exemption from WIN;

b. whether there is a need for medical, day care, homemaker, or other social services which could improve the Unemployed Father's employment potential;

c. whether the lack of recent work history is a significant factor in preventing the Unemployed Father from getting a job and is restricting his attempts to move into economic self-sufficiency;

d. whether other WIN employment or training strategies are more appropriate (e.g., WIN/OJT, suspense employment, institutional training);

e. whether a referral to a vocational rehabilitation agency is needed instead of or prior to assignment to a WIN component.

In the case of problem (c), the client would be judged appropriate for assignment to a work experience site. Otherwise, other treatments would be provided. Some clients would be inappropriate for work sites because they should not be in WIN altogether (problem (a)), while others would need specific training strategies (problems (d) and (e)).

Of the 3120 clients paper selected, only 1710 (55 percent) ever appeared for reappraisal interviews. Of those reappraised, 1033[6] were found appropriate for work experience. Ultimately, an evaluation of the reappraisal process must evaluate and interpret these numbers. The

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5 Division of Employment Security WIN Work Experience Initiative Regulations, effective April 11, 1978, but identical with the rules stated in the earlier WIN Amendments and the Implementation Procedures.

6 Based on the number of phone calls by local WIN offices for client assignment, Brandeis has 1015 men as appropriate. This discrepancy is explained by WIN staff changing the status of 18 men who were originally deemed appropriate.

numbers depend on various factors relating both to client characteristics and to actions taken by MDES/WIN and/or SAU staff. This section concentrates on the actions and procedures of the reappraisal process, to evaluate the nature of actual treatments as well as to consider the possibilities of error and variability in treatment. Appendix D will try to account more fully for the numbers on client flows.

The reappraisal process can be considered in three parts, each involving actions--and hence the possibility of variations in actions--by MDES/WIN or SAU staff. These parts are the call-in for the interview, the actual interview process, and the evaluation of the client's needs.

1. Call-in for Reappraisal

Table IV-2 presents a breakdown by region of the numbers reappraised in relation to those paper selected for the same early period of the program used in Table IV-1. One might expect that the process of calling in clients for reappraisal interviews would be relatively straightforward. However, this seems not to have been the case. It appears likely that one reason many paper selected clients never showed up for interviews is that they were never actually called in.

Table IV-2

Regional Differences in Unassigned Recipient Pool, Paper Selections and Reappraisals
January 1978-June 1978

Region	(1) Number Paper Selected	(2) Number Reappraised	(3) Percent Reappraised (2):(1)x100	(4) Percent Paper Selected[a]
1	274	173	63	44
2	195	71	36	15
3	122	111	90	21
4	107	81	75	16
5	181	118	65	23
6	675	190	22	61
Overall	1554	744	48	31

a/ Reproduced from Table IV-1 for convenience.

Source: MDES Progress Report to USDOL, August 24, 1978.

Typically, cases were called in by mail by the MDES/WIN staff. When the MDES/WIN staff that were hired through CETA were dismissed from WIN/MASSWEP jobs, responsibility for calling in the paper selected cases never was transferred to another MDES/WIN staff person. Consequently, many paper selected cases simply were abandoned and left untouched in files. The interviews done for the process analysis suggest that this accounts for the bulk of the 1410 clients who according to the state were paper selected and did not appear for reappraisal.

Next in importance was the fact some staff were reluctant to pressure their clients. This, in effect, is what occurred in region 6. There it became general policy that men who were paper selected would be given notice that they had 30 days to find jobs or accept other WIN treatments even though this was not prescribed by program rules. Only if they did not succed in that period would they be reappraised. More generally, it appears that in region 6, there was among the WIN staff substantial resistance to--even defiance of--MASSWEP. There were even suggestions to our process analyst that clients were encouraged not to cooperate with the program.

WIN and SAU personnel indicated in interviews for the process analysis that several clients had to be notified a second time before they appeared for reappraisal. Several men claimed to be in the process of obtaining medical exemptions while others said they were seeking jobs. However, where administrative enforcement of call-ins was known to be stringent because of the attitudes of a WIN staff person, it appears that virtually all clients eventually responded to call-ins. They did not want to jeopardize their AFDC-UF checks by failing to respond.

2. Staff and Client Interactions at Reappraisal Interviews

Although the reappraisal interview was to involve in each case both MDPW/SAU and MDES/WIN staff, simultaneously or serially, frequently

they were not both involved. In the early phase of MASSWEP, there were both simultaneous and sequential interviews. In some places, however, SAU workers never were available for this critical aspect of the program, while in others the problem with SAU staff developed as time progressed. The Brandeis process analyst found five (of 35) WIN offices at the outset in which there was no SAU staff and found an additional six with incomplete SAU coverage. In about eight others joint interviews were held; but when clients occasionally failed to appear, SAU workers sometimes became discouraged about traveling for the reappraisal. Although there was some friction between SAU and WIN staff when reappraisals were joint, cooperation was the rule. An illustration of the friction is provided by the frequency of comments by SAU workers that WIN staff were uninterested in finding "good" jobs for clients, while WIN staff viewed their SAU counterparts as coddling the AFDC-UF fathers.

What actually transpired at reappraisal interviews varied considerably, as far as could be gathered from interviews with WIN and SAU staff. The task of SAU staff was to assess a client's medical or psychological problems and need for social services; and then develop a plan for such services. The WIN staff were to assess skill deficiencies and other direct barriers to reemployment. The Brandeis process analyst found that some staff persons concentrated on client problems related to employability, while others concentrated on describing the MASSWEP program to the client. In the original design, the latter was not supposed to occur until a second interview and then only for persons judged appropriate and who were to be assigned to a treatment group.

Clients were not to be told about treatments that would not apply to them. However, second interviews frequently were not held because of the difficulty of arranging them.

3. The Decision on Appropriateness for Assignment to Work Sites

The reappraisal was intended to distinguish those appropriate for assignment to a work site from those needing other treatments. Those not appropriate would include both the clients least employable and those most suitable for other WIN treatments. Clients deserving exemption from the WIN registration requirement were excluded, as were those requiring prior medical services, vocational rehabilitation, or WIN/OJT. The appropriates were to be those basically employable, but who had not worked in a long time. They may have had barriers to employment, but in the opinions of the WIN and SAU staff needed only work experience to overcome those barriers.

In an attempt to determine the bases of decisions made by WIN and SAU personnel regarding client appropriateness for work sites, the Brandeis research group devised two forms listing possible barriers to re-employment facing AFDC-UF fathers, one to be filled out by the WIN worker and the other by the SAU worker conducting the reappraisal interview. Copies of the two "barrier" forms appear in Appendix B. SAU workers had to assess psychological and social service barriers, while WIN staff had to assess problems more directly related to employment prospects. Each also had to indicate whether any of the checked problems caused the client to lose his last job. The numbers and kinds

of barriers indicated can be related to the actual decision on a client--appropriate or inappropriate--to try to see how systematically reappraisal decisions were made.

Data from the state show 674 men found inappropriate out of 1710 reappraised, just under 40 percent. Unfortunately, WIN and SAU barrier forms were not submitted to Brandeis for many clients. Brandeis received 1218 WIN barrier forms, of which 35 percent were inappropriates, and 651 SAU barrier forms, of which 10 percent were inappropriates. Thus there was a definite tendency for barrier forms for inappropriates not to be submitted. Despite the participation of Brandeis researchers in two waves of training sessions and numerous mailings to workers, SAU workers in particular reported that they were unaware of the existence of the barrier forms; some who knew of them said they could not obtain any. WIN barrier forms on persons found inappropriate were not submitted at all from eight of the 35 local offices. Thus our analysis based on comparisons between appropriates and inappropriates should be interpreted with caution.

As can be seen in part A of Table IV-3, based on the SAU barrier forms, the most common barriers in the eyes of SAU staff to getting or keeping a job were medical/physical handicaps and transportation problems. Thirty percent faced only one barrier, while 54 percent of the 587 appropriates were found to face no barriers. This low incidence of barriers on SAU forms may reflect the fact that cases with serious alcoholic or medical problems often were screened out during paper selection. Moreover, several SAU staff persons had an

aversion to noting the existence of such problems. Interestingly, none of the SAU workers sent AFDC-UF fathers to receive medical care that was to be specially subsidized by USDOL funds allocated to MASSWEP, even though medical and transportation problems were the only barriers (among eight listed) that were mentioned with any frequency for the appropriates. Few, if any, SAU staff knew that the funds were available for medical and social service despite the fact that they were mentioned in training sessions. As can be seen in part B of Table IV-3, based on the WIN barrier forms, by far the most common barrier in the eyes of the WIN staff for both appropriates and inappropriates was "looked for work but could not obtain a job offer." This was followed by lack of education, skills, and references.

Again, while the data yielded by the WIN barrier forms must be interpreted cautiously in light of the fact that many local offices never submitted the forms for inappropriate cases--and 72 percent of these were submitted by six of the 35 local WIN offices--they suggest that the reappraisals were used to ferret out both the least and the most employable cases. First, 23 percent of the 333 inappropriate cases compared to only 12 percent of the 885 appropriate cases, were found to face no employment barriers. Moreover, while 34 percent of the inappropriate cases faced two or more barriers, 44 percent of the appropriate cases were in that position. It must be remembered that while some of the barriers represent difficulties not easily overcome, others may be treated. It should be expected that the appropriates should have employment problems, but that these problems are treatable, in particular by work experience.

Table IV-3

Barriers to Working

A. Barriers on SAU Worker Forms[a]

Barrier	Percent with Barrier	
	Inappropriates (N=64)	Appropriates (N=587)
1. Needs child care	11	3
2. Medical/physical (includes alcoholism or drug use)	48	24
--restricts whether can work	30	6
--restricts type of work	42	16
3. Transportation	27	20
4. Legal entanglements	11	7
5. Marital difficulties	11	3
6. Difficulty getting along with others	6	3
7. Tense and nervous	19	8
8. Depression	8	4

a/ SAU Barrier Forms were submitted for 651 men who were reappraised.

B. Barriers on WIN Worker Form[b]

Barrier	Percent with Barrier	
	Inappropriates (N=333)	Appropriates (N=880)
1. Lacks education	37	41
2. Little previous job experience	17	25
3. Lacks specialized skills	27	41
4. Lacks references	33	40
5. Difficulty in English	13	8
6. Difficulty reading or writing	18	17
7. Looked for work; did not obtain job offer	50	73
8. Wanted to look for work; lacked confidence	13	18

b/ WIN Barrier Forms were submitted for 1213 men who were reappraised.

Additional information provided by MDES/WIN personnel on the WIN barrier forms regarding why men were deemed inappropriate for work sites lends further support to the notion that the re-appraisals resulted in excluding some of the most employable as well as some of the least employable cases. The WIN staff had to check whether a client was suitable for a WIN treatment other than work experience. The options facing the WIN staff and their choices are listed in Table IV-4.

Table IV-4

Reasons for Being Deemed "Inappropriate" by MDES/WIN Interviewers

	Number	Percent
Vocational Rehabilitation	15	4
Obtain Medical Report	78	23
No Further Action	120	35
Other	131	38
	344[a]	100

a/ In 11 cases, two reasons were given, thus accounting for the additional 11 forms, as compared to the total of 333 inappropriates in this section.

In order to learn about what kinds of clients were judged inappropriate, written responses to "other" and "no further action" were analyzed. In all, 112 of the 344 reasons for being inappropriate (33

percent) were related to getting a job. Fifteen men were already employed; sixty-five men were expecting jobs or searching for jobs or were referred to jobs; and 32 men were in some type of training program. The second most common reason for being judged inappropriate was "medical problems." In addition to the 78 obtaining medical reports, 20 of the "no further actions" also had written responses indicating medical problems, adding to a total of 98 (28 percent) of the total number of inappropriates. As both WIN workers and SAU interviewers indicated, clients often said things like, "I can't work because I have a bad back."

Two last pieces of evidence come from data in the WIN barrier forms on the men's most recent spell of full-time employment. The average length of the most recent spell of unemployment was 25 months for the appropriates and 30 months for the inappropriates. However, the average hides the fact that the inappropriates included both more short term and more long term cases: nine percent of the inappropriates were out of work five months or less by the time of their reappraisal and 18 percent were out more than four years; for the appropriates, the corresponding numbers were two and ten percent. The whole distributions of the two groups by the length in months of their last full-time jobs are shown in Table IV-5.

TABLE IV-5

Distribution of Men Undergoing Reappraisal
By Number of Months Since
Last Full-time Job

Number of Months Since Last Job	Inappropriates N=319		Appropriates N=841	
	Number	%	Number	%
0-5	30	9	21	2
6-12	69	22	235	29
13-24	75	23	280	33
25-36	50	16	138	16
37-48	37	12	82	10
49-60	26	8	35	4
61-72	13	4	17	2
73+	19	6	33	4
TOTAL	319	100	841	100

Source: 1160 WIN Barrier Forms. (There was no information on 58 other Barrier Forms)

The reappraisal process was intended to find the most suitable WIN treatment for each client, with work experience just one of the options. The fact that about 30 percent of the cases were found inappropriate for work experience also suggests that they did not belong in the unassigned recipient pool on a prolonged basis, even before MASSWEP was introduced. Either they should have been placed in one of the standard WIN treatments or they should have been off welfare altogether. One of the benefits of MASSWEP thus was its role in cleaning the unassigned recipient pool. Of course, inappropriate clients were found at the paper selection stage too, so that the two screening operations combined, no doubt, did cleanse many from the pool. The remaining 70 percent apparently did belong in the pool; at least WIN workers did not find any other treatment preferable to work experience, so that in its absence many of these clients would have remained in the unassigned recipient pool.

C. Assignment and Experimental Treatment Stage

Following their first reappraisal interview, men judged appropriate for assignment to work experience were to be divided on a random basis into three groups. MDES/WIN staff were to call Brandeis to get for each client a random assignment to one of the three groups, the Control Group, Experimental Group I, and Experimental Group II. A critical factor was that men in the two experimental groups who found regular jobs were able to retain eligibility for Medicaid as long as their earnings did not reduce their AFDC-UF benefits to zero. If earnings rose so high as to make benefits zero, then, as is customary, eligibility for Medicaid was lost after working for four months.

Using for the balance of this chapter data gathered by Brandeis, of 1015 men who passed the reappraisal, 290 were assigned to the Control Group and Experimental Group I combined. Among the 725 men in Experimental Group II, only 256, or 35 percent, ever reported to work sites; of the remainder only 60 recalled being asked to go to sites. Moreover, most men who ever appeared at work sites worked for very short periods: according to records on attendance at work sites, only 27 completed 13 or more weeks at a work assignment. These developments in MASSWEP are discussed in this section.

1. Staff and Client Reactions to Work Assignments

Attitudes among WIN personnel varied across offices, within offices, among individual WIN staff, and between MDES/WIN and MDPW/SAU staff. In one office, WIN counselors generally were favorably disposed towards MASSWEP and annoyed that MDES/WIN did not have more to say about the role of welfare; "welfare is soft," Brandeis was told. Indeed, the MDPW/SAU worker in this team said MDES/WIN appeared to be "pushing a man to take a job" without considering whether the job had benefits or stability. In another office, a WIN interviewer, hired through CETA, saw his role in MASSWEP as "a challenge." "I tell it like it is," attributing his "success" to his personal ability to communicate with and motivate people. In a third office, the staff reported that MASSWEP gave them some "clout"; but, in response to an early administrative "quota" on making assignments to work, called in

only "those we wanted." The men were confronted with a description of the program and told that "assignment to the experiment is not determined locally." Most vigorous in pursuing the men were MDES/WIN staff in a fourth office, where one interviewer received the distinction of the highest number of placements on work sites. In contrast, the regional MDPW staff in this area summed up the performance of MDES by saying, "They are only interested in statistics; they don't focus on clients' problems. We're concerned about permanence." Another SAU worker reflected a similar position: "Will the man be different after 13 weeks at a work site with no--or very little--training?" It would be incorrect to suggest that all SAU staff were opposed to compulsory assignments. One described himself, for example, as "the most conservative social worker you will ever meet"; and another was outspoken about welfare "chislers" and embraced MASSWEP with the cry, "give him the WEP."

In addition to those who supported mandatory work experience were many MDES/WIN staff who opposed it. One told us, "we don't want to sanction someone who doesn't want to work for nothing." While there were several local WIN teams and even an entire region that never enforced mandatory work assignments, those that did frequently reported being disrupted by the court injunction. For them, MASSWEP was converted from a mandatory to a "voluntary" program. As previously noted, at the time of the injunction during the tenth week of MASSWEP, 66 men were on work sites and had to be notified that they no longer

were required to work. (Of the 66, state data show that 42 voluntarily remained at their work sites.) In some offices, staff were watched very closely by legal groups. After the injunction some offices noticed less pressure by higher administrators to come up with more participants. One WIN interviewer expressed concern about the legal aspects of MASSWEP. He told us, "We have the highest rate for winning litigation procedures in the state. Perhaps it's because we carefully and profusely document everything we do." This interviewer gave clients little information about the program during the reappraisals. He told the men that "this is an assessment before a special state program starts up," and used the approach, "what would you like if you went on a site?"

Although the attitudes of staff persons changed during the course of the program, so did the staffs and the general atmosphere change. Based on our interviews with a limited sample of WIN workers, early in the program, two-thirds of the SAU staff and three-fourths of the MDES/WIN staff favored mandatory assignments for some men. There was extensive interest in a permanent workfare program, and over 50 percent of both groups did not want it to be voluntary. As the program progressed, our process analyst detected a tendency for more staff persons to favor a selective mandatory program, giving local case managers the power to determine when work experience would be obligatory for the client. Towards the end of MASSWEP, most SAU and WIN staff did not want to be obligated always to utilize mandatory assignments for all appropriates.

The concept of "mandatory" work experience was generally approved if the mandatory aspect was at the discretion of the staff person. WIN and SAU staff felt that when a man was unemployed for a long period, he sometimes became afraid of taking a chance and got depressed. In such cases, as well as in those in which men obviously were work resistors, it could be useful to force a man into a work situation. However, few staff people felt that 13 weeks was a realistic work experience that could change attitudes and habits. They preferred a more careful assignment that might lead to a job. Building a man's self-esteem was a critical goal for the first group. For other men who the staff felt were abusing the system, tightening and simplifying the adjudication system was seen as important. These staff people scoffed at the mechanism of 60-day counseling and the process that put a man off AFDC-UF for 90 days, permitting him to return at that time. WIN staff often said that welfare personnel were slow in deregistering clients who abused the system.

Interviews conducted for the process analysis revealed that several local WIN teams did use some discretion in making assignments, pursuing first those AFDC-UF fathers suspected of abusing the system. In many cases, local staff said that the availability of Medicaid, the 100-hour rule, the generosity of welfare benefits, and the psychological security offered by welfare accounted for long-term unemployment. For some of the men who were unemployed for reasons like these, WIN staff wanted badly to use MASSWEP to encourage a quick return to work.

An additional concern of WIN staff related to the fact that MASSWEP was an "experiment." One person, reflecting this view, told the AFDC-UF men, "I'm simply doing my job and am required to inform you about MASSWEP." Another was openly hostile to the idea of an "experiment." At the Spanish Speaking Center, the staff though it unrealistic to conduct an experiment in the close-knit Spanish community. "Word spreads quickly; can you imagine a man assigned to Experimental Group I telling a work experience man that at the end he'll have the same thing without showing up on a site?"

As for client reactions, reappraisal interviewers reported that by and large many men listened to what they had to say and accepted the program as just another WIN rule. One WIN interviewer described men as "passive" although very experienced about WIN. But there were exceptions. An SAU person described the men as "program wise" who do "a lot of game playing...although they promised they'd give their 100 percent to this new program, it was mostly words." Yet another WIN interviewer, who appeared to be very much in charge of the program, said that "no one was pleased" when they heard about MASSWEP and two clients were violently opposed. An experienced interviewer reported that "men didn't understand WEP but they sure understand 13 weeks of work." She reported that several replied, "I won't go to work for nothing." Her way of handling this was to coax them: "I tell them if they go, I can then tell an employer, 'Here's a man who has shown he can work.'" Another interviewer encouraged men to go to a site while assuring them they "didn't have to stay," that she would work closely with them to find a suitable job and take advantage of the 30 and 1/3. One interviewer had an outright refusal to go to

work from a man who both threatened and went to Legal Services. This interviewer was told by the MDES/WIN regional coordinator not to pursue this case. By contrast, a local WIN team reported that there were one or two men eager to participate: "they were happy to get their foot through the door at Public Works," hoping they would get a job with a good wage. However, another man objected to going on a site because he "didn't want to be known as a welfare recipient."

2. The Modification of the 100-hour Rule and Use of 30-and-1/3

The MDES/WIN staff greeted warmly the modification of the 100-hour rule. In the view of SAU and WIN staff, the two critical obstacles to work created by the AFDC-UF program are those resulting from the 100-hour rule and the fact that Medicaid benefits also are lost when a man works for four consecutive months for 100 or more hours per month.

In considering the possible impact of the modification of the 100-hour rule, it is important to state precisely how the waiver worked and was phased out. Each client finding a regular job was permitted to have his benefits calculated using the 30-and-1/3 disregard only for six months. On March 15, 1979, WIN staff at all levels were informed of the procedures for the phasing out of the waiver. First, men who had found jobs were entitled to complete their six months. Secondly, men who had been in the two experimental groups for at least 21 weeks and who had not found regular jobs by April 30, 1979, no longer would be eligible for the 30-and-1/3. The few men still at work sites at that time had eight weeks from the time they left those sites to find regular jobs and be entitled to 30-and-1/3.

In the early months of MASSWEP, it appears only irregular efforts were made at informing men in the two experimental groups of the modification of the 100-hour rule. Following the lifting of the injunction, when it became attractive to administrators to find regular jobs rather than MASSWEP work sites for clients, the rule modification was better publicized. Support for placing men in regular jobs and for greater use of the 30-and-1/3 incentive emerged at the state level. Word began to filter to regional coordinators about this change in emphasis and new training sessions were scheduled for late 1978 and early 1979. Encouragement to use 30-and-1/3 as well as the need to assign men to work sites was emphasized at these sessions. New leaflets describing 30-and-1/3 in English and in Spanish were printed and distributed to local teams. Both WIN and SAU staff, perhaps familiar with 30-and-1/3 as it applied to women, were instructed in the required paper work to assure payment of the 30-and-1/3 incentive. Frequently, as the Brandeis process analyst went to local offices later in the program, interviewers commented on a shift from a "get bodies" strategy to a "get jobs" policy. WIN workers claimed that 30-and-1/3 often could be effective in encouraging men, particularly those with low skills and large families, to take low paying jobs, especially for short periods of time.

Only about half of those men completing the Brandeis follow-up questionnaire said that someone had explained to them the experimental replacement of the 100-hour limitation by the 30-and-1/3 incentive. We know from interviews with WIN staff that the 30-and-1/3 was not emphasized in the early reappraisals. Typically (in 85 percent of the cases), the explanation was made by a WIN staff person, although the welfare manual instructed that the SAU spell out the detailed explanation. When asked whether this experimental change influenced their decision to take a job, 20 percent of the clients in Experimental Group I and just over 10 percent of those in Experimental Group II said that it had. It could be that the rule modification was more in the minds of persons in the former group because the waiver (and associated change in Medicaid eligiblity) was their sole experimental treatment.

Finally, it should be noted that one of the most frequent comments in interviews with WIN and SAU staff was the importance of Medicaid benefits to AFDC-UF fathers and their families. It is difficult to sort out how much the staff saw it as important as opposed to how much the men themselves saw it as important. MASSWEP participants apparently asked frequently how their Medicaid benefits would be affected by the program. Moreover, staff persons reported that they urge the AFDC-UF men to discuss the question of medical benefits when they interview for regular jobs. Staff noted that Medicaid coverage for four months after employment is important since medical benefits on jobs, if they exist, rarely start in the first month of employment. SAU and WIN staff suggested that a man might take a job without medical benefits

or without pregnancy coverage and his wife would report the man out of the home in order that she receive Medicaid for herself and children. WIN staff also noted that small employers are sympathetic to the need for medical benefits andagree to permit the AFDC-UF father to return to welfare to obtain medical coverage. The "little green (Medicaid) card" is considered a valuable possession.

3. Experience at Work Sites

a. Job Development and Work Sites

The development of work sites went on as part of the early planning (described in Chapter III) for a "workfare" program. Thus, long before MASSWEP officially began in January 1978, the Massachusetts Executive Office of Economic Affairs (EOEA) solicited, by mail, employers in the foluntary and public sectors throughout the Commonwealth. EOEA and WIN staff (particularly CETA employees) followed up the mail contact with visits to potential sponsors to secure job slots. A sufficient number of job vacancies were offered in five of the six WIN regions to meet the needs of the proposed "workfare" program. However, some of these promised jobs did not materialize and were not available to the (13 week) MASSWEP program that evolved.

Very important to the job development process was the use by WIN staff of their personal contacts with various job sponsors. Occasionally, an AFDC-UF father, under pressure to find work, developed his own work site: for example, one father worked at a music conservatory

where he had been employed under CETA. In the early months of MASSWEP only three or four local WIN teams reported difficulties in finding work sites. Boston teams, however, reported a chronic shortage of work sites except for the availability of jobs at cemeteries and public works. Later in the program, some difficulties arose in developing jobs because several potential employers were concerned about the impact of the court injunction.

Finding "meaningful" work sites, ones offering substantial opportunities for on-the-job training and/or the prospect of regular employment, was a more serious problem throughout the program. Some WIN staff members, deeply interested in developing "meaningful" work sites for clients, were frustrated by the early pressures to "get bodies" into sites. This policy sometimes meant a disregard for the quality of the work site. However, opportunities did arise at times for assignments to slots offering the redevelopment or preservation of skills. One AFDC-UF client with a background in restaurant cooking was assigned to cook for a pre-school program in a local church; he eventually was hired on a regular basis by the pre-school program. At the other extreme, a few dozen men were sent to rake leaves in a local cemetery. Disgruntled by this, most of them worked for barely one day before leaving and not returning.

In all, there were somewhat less than 150 different work sites utilized by the program. The employers involved are divided in Table IV-6 into seven categories. Brandeis interviewed a cross-section of sponsors. The single largest group of assignees worked in various

TABLE IV-6[a]

Distribution of Assignees to Work Sites, by Type of Employer

Type of Employer	Number of Assignees Interviewed by Brandeis	Number of Sponsors Interviewed
1. Community Centers (YMCA, Girls Club, Red Cross, etc.)	66	20
2. Social and Rehabilitation Organizations (Goodwill, Alcohol and Drug Centers, etc.)	7	3
3. Hospitals, Nursing Homes, Health Centers	14	8
4. Schools and Churches	26	11
5. State and Local Offices	17	5
6. Municipal Garages, MDPW Sanitation, Roads	27	10
7. Other (Cemetery, ASPCA, etc.)	18	3
TOTAL	175	60

a/ This table is based on the sample of men who went to work sites. Brandeis targeted these interviews to obtain a cross-section of both sponsor's and men's attitudes toward the work site experience. In order to maximize the varieties of work-site experience, Brandeis may have over-sampled community agencies and under-sampled Massachusetts Department of Public Works (MDPW) and cemetery sites.

types of community centers; along with the centers, schools, and local public works agencies employed about two-thirds of the work site participants who were interviewed by Brandeis.

About 65 percent of the jobs were classified as requiring no skill; these included janitorial, highway maintenance, and leaf-raking work. Other jobs, requiring some skill, included equipment maintenance, painting and cooking jobs. On the whole, the MASSWEP employees were viewed as equivalent to those regular employees who had or were performing the same duties. Yet about one-quarter of the employers perceived the MASSWEP men to have poorer attendance records and to be less motivated than their usual employees in these jobs. The men's reported reactions to their work were often positive. Almost everyone got along well with fellow workers; only two did not. Supervisor relationships were seen as "very good" by 112, "O.K." by 37, and only four men reported "not so good" relationships with supervisors. Twenty-five percent of the men who went to sites and who were interviewed felt they "learned something" or a "great deal" from their jobs while 75 percent felt they "learned little" or "nothing." The original intent of the project was not to provide training, but was to develop good work habits. A typical comment by work site men regarding what was learned was made by a maintenance worker at a courthourse, who said, "it's not giving me any training that I can use in the future to get a good paying job."

b. Client Attendance at Work

Attendance at work sites was reported by job sponsors on time cards. These cards were used by WIN interviewers who then submitted actual Time Payment Cards for 256 (35 percent) of the 725 men in Experimental Group II. This indicates that these men actually appeared at a work site for one or more hours at some time during the fifteen month program. Two questions arise. Why did 65 percent of the potential assignees never appear at a site? How stable was the participation of the 35 percent who did appear?

The data obtained in the follow-up interviews indicate that 72 percent of those who did not attend a site claimed they never were "...asked to begin a work site at some time." (Follow-up interviews were completed by 45 percent of the 469 men who never went to a work site.) Among the 28 percent who were asked to go but did not, about one-third said they got jobs on their own, almost half of these part-time; while only 15 percent would say that they did not go because they did not like the program.

Going back to the larger sample of data from time cards, Table IV-7A and B shows that 27 men worked for 13 or more weeks at an assigned work site. The standard MASSWEP work assignment was to last for 13 weeks during each of which a client would work three days, a possible total of 39 days. Attendance might have been recorded if the man appeared for as little as one hour.[7] One man, for example,

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[7] On the other hand, some men may have appeared at the work site and this was not recorded.

Table IV-7[a]

Distribution of Those Appearing at Work Sites

Part A
BY DAYS WORKED

Number of Days[b]	Number of Men	Percent of Men
1	28	11
2-9	70	27
10-21	60	23
22-30	32	13
31-38	27	11
39	8	3
40 or more	31[e]	12
TOTAL	256[c]	100

Part B
BY WEEKS WORKED

Number of Weeks	Number of Men	Percent of Men
2 weeks or less	70	28
3-4	42	17
5-6	27	11
7-8	29	12
9-10	21	8
11-12	35	14
13	16	6
14 or more[d]	11[e]	4
TOTAL	251[f]	100

a/ WIN teams used the work sponsor time cards to fill out time "payment cards." These payment cards, which covered two weeks of work, were submitted to the WIN central processing unit and are the source for data presented in these tables.

b/ In terms of the total number of days, some men may have worked as little as one day in a two week period whereas others may have worked as many as ten days in two weeks.

c/ The state also has information based on insurance forms which indicate that 301 men may have at some time attended sites.

d/ Interviews with men and with WIN staff revealed that among men who worked on a site 14 or more weeks were three men who hoped to get a permanent job and two men who went to a second site after the first did not work out.

e/ The number of men who worked 40 or more days and those who worked 14 or more weeks may appear to be inconsistent; but remember that men could have worked more than three days per week reaching a total of 39 days in less than 13 weeks.

f/ Data are missing for five individuals.

allegedly fainted on his arrival at a site, never to return. Interviews with a few men who went to sites for less than a day revealed comments like, "I showed up (at the cemetery) and everyone was just standing around. There was nothing for me to do, so I left." One wife told the Brandeis interviewer that her husband had to paint highway dividing lines in the high-speed lane of a throughway. She felt that this was too dangerous for him to continue, so he left after one day. In addition, a few men who went to sites at the very beginning of the project, before the process of record keeping and tracking had been implemented, may not have been tracked by the time cards.

c. Work Sponsor Reactions and Possibilities for a Permanent Program

While MASSWEP was an experiment largely intended to gauge client reactions to this type of employment-training program, it is of obvious interest to inquire whether the reactions of employers to the program shed light on its feasibility as a permanent part of the welfare-employment system. Employer reactions will vary depending on how their experience with MASSWEP compared with their expectations. Some employers were interested only in getting "free help," and had no desire to train unskilled labor, or to complement their workers with capital equipment, or to integrate MASSWEP participants with their regular employees. Other employers may have carved out special vacancies which would be costly to fill with a new worker.

While many of the former type of employer were content with their experience, there was some displeasure among the latter type. Employers were upset in some instances when they realized that assignees could--and did--leave at any time, often to find another job. Especially prior to the injunction, some assignees reported to their sites reluctantly, and employers found this problematic. Some men, according to several work sponsors, needed prior services, as in the case of an alcoholic. (Of course, such cases should have been assigned to prior treatment in the reappraisal.) Lastly, irregular scheduling was a problem for some employers who could not easily take men for but three days per week. In sum, there was substantial variation in employer responses, some of whom would remain in the program and others of whom never again would participate.

d. Sanctions

Sanctioning in the WIN Programs means denying an individual's family his share of its monthly benefit from AFDC-UF. Sanctions may be the outcome of noncompliance with a requirement of the WIN Program. In MASSWEP, if an individual failed to respond to a call-in request, or failed to accept assignment to a work site, or did not return to a work site for an unacceptable reason, then his WIN counselor was to begin the procedure which could result in sanctioning. Upon being warned about noncompliance in the WIN Program, an individual is taken through a 60-day counseling period, the first step of the adjudication process that may result in sanctions. If an individual fails to comply during this period, a WIN staff person may recommend the cessation of his

portion of the family's benefit. For sanctions to be brought, the MDPW staff must concur with and then implement the judgment of the MDES/WIN staff person.

Neither 60-day counseling nor sanctions seem to have been used extensively, but they were used at a somewhat higher rate for Experimental Group II than for the Control Group. The best data available to Brandeis on this matter come from the "5-97" forms completed by WIN staff. In Table IV-8 below, the experimental and control groups are compared regarding the numbers and proportions sent into 60-day counseling and, eventually, in some cases, sanctioned. In the upper half of the table, it can be seen that 4.8 percent of Control Group, which received no experimental treatment, was sent into 60-day counseling. The proportions in the other groups should be compared with this base. Experimental Group II is divided into two parts, those who did and did not go to work sites. Of those who went, 13.4 percent were sent into counseling following an allegation of noncompliance. Among those in Experimental Group II who were not sent to sites, only 2.5 percent were sent into counseling. For the whole Experimental II, just under 10 percent were sent into sanctioning. Thus, the incidence of noncompliance resulting in counseling overall was about 5 percentage points higher in Experimental Group II than in the Control Group. There also is an observable pattern of differences among the six WIN regions. In region 6, for example, the region in which there appeared to be the greatest staff resistance to MASSWEP, fewer people were sent into counseling than in the other regions.

TABLE IV- 8

Cases Sent to Sixty Day Counseling and Sanctioned by Experimental Group and WIN Region

Procedure	Group	WIN REGION (1)	(2)	(3)	(4)	(5)	(6)	TOTALS
		Number and (Percent) Experiencing Procedure						
60 Day Counseling	Control	1 (4)	0 (0)	2 (11.1)	0 (0)	2 (7.7)	0 (0)	5 (4.8)
	Experi-mental I	1 (4.5)	0 (0)	1 (5.3)	0 (0)	2 (6.7)	0 (0)	4 (4.0)
	Experi-mental II (sites)	9 (11.4)	9 (19.1)	11 (18.6)	6 (12.8)	12 (15.2)	2 (3.6)	49 (13.4)
	Experi-mental II (not on sites)	1 (3.3)	1 (3.3)	1 (1.9)	0 (0)	2 (5.0)	1 (2.3)	6 (2.5)
Sanctioned		1 (4.0)	0 (0)	2 (11.1)	0 (0)	1 (3.8)	0 (0)	4 (3.8)
		2 (9.1)	0 (0)	2 (10.5)	0 (0)	2 (6.7)	0 (0)	6 (6.0)
	(sites)	10 (12.7)	5 (10.6)	8 (13.6)	2 (4.3)	5 (6.3)	2 (3.6)	32 (8.7)
	(not on sites)	1 (3.3)	0 (0)	3 (5.8)	1 (2.4)	1 (2.5)	2 (4.5)	8 (3.4)

Of course, not all of those sent into counselling were ultimately sanctioned. Experimental Group II again has a sanctioning rate a few percentage points above the Control Group. The pattern of regional differences is about the same here as it was for 60-day counseling.

e. Job Search During Work Site Assignments

During the thirteen week on-site work experience component, participants were to spend two days per week searching for unsubsidized employment. The regulations for MASSWEP called for MDES/WIN staff to "...provide intensive job search assistance, job development, and employment counseling services to more unemployed fathers than previously anticipated."[8] While the AFDC-UF fathers were to combine their own resources with the assistance provided by MDES/WIN, they were not to be given the type of training in job search embodied in "job clubs" or similar direct job search programs.[9]

Job search services may have been offered both to the 256 men appearing at MASSWEP work sites as well as to the 469 who did not, but the data gathered by Brandeis on job search services at this stage of the program relate only to the former group. A large fraction of men who appeared at work sites were interviewed and it was during this interview that men were asked about the job services that they were given during this phase of MASSWEP. (Men in Experimental Group I also may have received special job services, but Brandeis did not gather data to capture this possibility.)

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[8] MDES/WIN Work Initiative Regulations, effective April 11, 1978.

[9] Robert Wegmann, "Job Search Assistance: A Review," Journal of Employment Counseling, Vol. 16, No. 4, December 1979.

These interviews with men working at sites combined with data gathered by Brandeis through interviews with MDES/WIN staff, suggest that the job search component of MASSWEP was a very modest effort. This should not be entirely surprising, since if but 27 men appeared at a work site for 13 or more weeks, not many men were on sites long enough to receive substantial related job search assistance. In the early stages of MASSWEP, 17 of 21 WIN staff said they gave extra time to AFDC-UF fathers. For a while, the WIN staff documented the job search efforts of AFDC-UF in order to substantiate claims for special stipends to support job seeking. After some time, almost no interviewers documented job search efforts. (In these interviews, WIN staff indicated that they gave extra assistance to some men who were not working at MASSWEP sites.)

On the client side when asked what they did during "...the two last days that you did not work on the WEP position," almost two-thirds of the respondents said they were searching for jobs. Interestingly, about 7 percent said they were working on those two days. Among the two-thirds who were seeking jobs, only about one in six said they had spoken to their WIN counselor during those two days. Others contacted private agencies, neighborhood agencies, employers, friends or looked at want ads. Among the one-third not seeking a Job during the two days, various reasons for not searching were given, the most frequent one being "busy doing other things;" several admitted to being afraid to lose their welfare benefits by getting a job. A last indication that the job search component of MASSWEP was

a modest effort came when the respondents were asked about the frequency of meetings with WEP counselors as part of MASSWEP. One-third said meetings were weekly or even more frequent; about one-fourth said rarely or never.

D. Conclusion

MASSWEP was a combination of a screening program and mandatory work experience for those found to be appropriate (together with a test of the 100 hours limitation). The screening was applied to men in the unassigned recipient pool and consisted of two steps, the paper selection and the reappraisal interviews. If performed reliably, the screening would provide a profile of the WIN unassigned pool. We found numerous problems in implementing both steps of the screening process. There was considerable variation in treatment across regions and local offices. While some was justifiable, some no doubt constituted error. An administrative review of the two screening processes combined found an error rate of 10 to 13 percent.

Given the errors in screening, the profile obtained of the unassigned recipient pool must be viewed with caution, but may nevertheless reveal some of its characteristics. MASSWEP was initiated in part because large numbers of men were remaining in the pool for prolonged periods. The paper selection was intended to identify those unemployed at least six months, who had been in WIN at least three months, who had been referred to CETA, and who were not obviously suitable--at least on paper--for other WIN treatments. Beginning with an initial unassigned recipient pool of 5228, the number paper selected

was 3120. This narrowed the pool to the long-term unemployed--those of principal interest to MASSWEP. Those paper selected were not homogeneous and were screened further in the reappraisal interviews. Some of these were long-term unemployed because they were barely employable, because of medical problems or alcoholism, for example. Others were readily employable, but by chance or choice had remained unemployed for a long period. (Some were already working without WIN staff knowing it.) The reappraisal interviews screened out both those most and least suitable for employment and sent them to other WIN treatments or removed them from WIN altogether. The remainder were judged appropriate for work experience. We do not have good information on how many of those screened out were either most or least employable, but altogether 1033 were found appropriate, leaving 2087 from the original 3120 paper selected who were either not reappraised or screened out. These clients, roughly two-thirds of the paper selected, were the long-term cases in the unassigned recipient pool who could (or should) have been receiving other WIN treatments or who should not have beeen on welfare altogether. This may be taken as an indication of the need for a screening process: the unassigned recipient pool absorbed far too many clients who should have been treated using the array of WIN treatments available before MASSWEP.

On the other hand the screening process revealed 1033 appropriates whom WIN staff judged suitable for work experience. While it might have been possible to place some of these in other WIN treatments, the WIN staff felt that work experience would be more suitable for them. If we trust these judgments, apparently the expected payoffs from the standard WIN treatments would not be large for these men.

Without work experience, these men most likely would have remained in the unassigned recipient pool. The screening process thus did eliminate large numbers of long-term unemployed men from the WIN unassigned recipient pool. However, many men probably would have remained in the pool in the absence of the work experience treatment. It remains to the impact analysis to evaluate whether the work experience treatment was indeed worthwhile for the men assigned to it.

As for the operation of the work experience treatment, the process analysis found serious difficulties. Of the 1033 men found appropriate for work experience, our experimental design selected 754 randomly to be subject to this treatment. Of these, 256 ever appeared at designated work sites, while only 27 spent the planned 13 weeks at their work site. Many spent only one or two days at their assigned work sites. It is true that many who did not go to work sites found jobs on their own and that the assignment to a work site "smoked out" some clients who already had jobs. However, many others were simply never assigned. Indeed the implementation problems in getting men to and keeping them at work experience sites creates a problem for evaluating the impact of work experience. Similarly, the modification of the 100 hours limitation did not operate smoothly in that many of those eligible for 30-and-1/3 were not informed of this fact in the early part of the experiment. These difficulties that arose in implementing the program will have to be taken into account in evaluating the impacts of the experimental treatments.

THE IMPACT SECTION

Who were the participants in the Massachusetts Work Experience Program and what effects did the program have on them? There were very few systematic differences in the demographic characteristics of the three experimental groups to which the men were randomly assigned. These characteristics are summarized in Appendix C. The next two chapters concentrate on the impact of the experimental treatments on the labor market and welfare experiences of the men who participated in the experiment.

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CHAPTER V

Effects of Experimental Treatments on Labor Market Experiences of Clients

One of the first issues considered when evaluating a welfare-related program is whether it is effective in getting clients to work. However, there are numerous other dimensions of labor market experience that can be affected. If a program does succeed in getting clients into jobs, then attention should be given to the quality of jobs found--how long they last, how much they pay, and what kind of work they involve. In addition programs may have subtle psychological effects on clients which for better or for worse affect their future performance in labor markets. This chapter is concerned with the effects of MASSWEP on these various measures of employment behavior.

Simply stating the questions leaves open the matter of how to study them. We shall rely heavily on our experimental design to test how people confronted with the experimental treatments compare in various aspects of labor behavior to those in the Control Group. However, the variations within our sample suggest the desirability of controlling for demographic and work history characteristics in addition to experimental treatment. The details of how we control as well as what we learn will be described as we proceed. However, some features of our data sources will be considered as background. Then we shall proceed to discussions of the effects of the experimental treatments on the probability of getting a job, on job search behavior, on the quality of work found, and on clients' attitudes toward work in general and the experimental program in particular.

A. Data Sources on Work Experiences

Three sources of data were available to us on labor market experiences during and after the experimental treatment. First, we have the 5-97 forms filled out by MDES/WIN each time the WIN status of a client changes. Since a client's WIN status changes when he gets a job, these forms provide information on work records. We collected all 5-97 forms available in MDES files on all clients in the three treatment groups for at least the nine month period beginning with the date of reappraisal of the client. (Since reappraisal dates were staggered, and since we collected the forms only on periodic visits to MDES offices, for some clients we collected forms beyond the relevant nine month interval.)

Second, we administered a follow-up questionnaire which was mailed to all 1015[1] clients passing reappraisal at their WIN listed addresses nine months after the date of each person's reappraisal. Three weeks after the questionnaire was mailed out, a post-card reminder was mailed out to those who had not yet returned the questionnaire. These procedures led to the return of 37 percent of the questionnaires. Those still not responding were then contacted by phone and/or interviewed personally. In some cases, especially for clients in locations not easily accessible, the interview was conducted by phone. Table V -1 summarizes the results of the interview process. Note that 313 were not accessible. Of the remaining 702, interviews were completed with 542, yielding 535 usable questionnaires or a response rate of 76 percent. Relative to the original 1015 this amounts to a rate of 53 percent.

Third, we administered face-to-face interviews to a sample of those who had attended MASSWEP work sites during or shortly after their time

[1] The State reported 1033 men to be appropriate for MASSWEP. However, WIN staff changed the status of 18 men prior to our mailings.

TABLE V -1

Response on Follow-up Questionnaire

	NUMBER	
Completed Questionnaire		542
1. By mail	337	
2. By personal interview	164	
3. By phone	41	
Not Accessible		313
1. No phone or unlisted	232	
2. Moved	49	
3. Left wife	12	
4. Language	15	
5. Drunk	5	
Apparently Accessible, But Not Interviewed		160
1. Phone contact said would send/did not	15	
2. Refused	44	
3. Several attempts to contact (3 or more times)	101	
Total		1015

at the site. These personal interviews provide detailed information about clients' short run responses to the program. They are used particularly to study clients' attitudes. However, since they were not given to clients in the Control Group, they are not used to study the experimental effects on work effort. For this purpose we must use the 5-97's and the follow-up questionnaires.

Having two independent data sources has advantages, but also creates difficulties. The 5-97's ostensibly cover all clients, but there is reason to suspect their completeness. The information on follow-up questionnaires may be more reliable, but with many not turned in, distortions could result. Consider first problems with the 5-97's. We are interested primarily in the information these contain about clients finding jobs. The problem is that WIN offices in fact have imperfect information on the extent of client work. Information reaches a WIN office either as a result of direct contact with the client, or in some cases from the welfare payments office. In either case there may be long lags between the time the client takes a job and the time this fact is recorded on a 5-97 form. Indeed it is likely that some clients conceal their jobs from both WIN and welfare offices as long as possible. If there are lags or concealment, the extent of job finding reported on 5-97's during a limited time interval will understate the true amount.

With respect to follow-up questionnaires, the problem relates to who returned them and whether there is a pattern in returns that could distort results. This requires examining whether the return rate is related to particular variables. In the early phase of questionnaire returns, it

became apparent, for example, that the response rate was particularly low among clients in the Control Group, many of whom were unaware that they were participating in MASSWEP. Since this group was small to begin with, we wanted to get enough returns to have a reasonable sample size. Thus personal interviews for the follow-up were concentrated disproportionately in the control Group. In the end we achieved a reasonably even distribution across treatment groups, with the figures summarized in Table V -2. The table also gives the breakdown within Experimental Group II between those who did and did not attend MASSWEP sites.

TABLE V -2

Response Rates by Treatment Group on Follow-up Questionnaires

Treatment Group	1 Group Size	2 Number of Returned Questionnaires	3 Percent (2):(1)x100
Control	150	86	57
Experimental I	140	77	55
Experimental II--Total	725	372	51
a. Attended MASSWEP site	256	148	58
b. Did not attend MASSWEP site	469	224	48

Perhaps of greater interest is whether returns were related to finding jobs. Since information from the 5-97's supposedly applies to all 1015 clients, we can compare the percent finding jobs based on 5-97 data between those who did and did not return follow-up questionnaires. The percents, broken down by treatment group, appear in columns 2 and 3 of

Table V-3. Column 4 presents comparable percents based on the data in the follow-ups. Relying first on the 5-97 data alone, it appears that those who return follow-ups do find jobs at a slightly higher rate than those who do not. The pattern in job finding is generally consistent across treatment groups with the lowest rate in the Control Group in columns 1 and 2 and in Experimental Group I in column 3. The rate is generally a few points higher among returners (column 3 in comparison to 2) although the difference amounts to 10 percentage points for those in the Control Group.

Much more dramatic than the difference between those who do and do not return the forms is the difference between the two data sources which may be seen by comparing columns 3 and 4. The pattern of differences between treatment groups is similar for both data sources. However, the reported

TABLE V -3

Percent Finding Jobs: Comparisons Between Data Sources and Samples

Data Source	(1) 5-97 Forms	(2) 5-97 Forms	(3) 5-97 Forms	(4) Follow-up Questionnaires
Sample (Sample Size)	All Clients (1015)	Clients Who Did Not Return Follow-ups (480)	Clients Who Returned Follow-ups (535)	Clients Who Returned Follow-ups (535)
Treatment Group				
Control	38.7%[a]	32.8%	43.0%	57.0%
Experimental I	38.6	38.1	39.0	55.8
Experimental II--Total	42.0	39.1	44.9	62.6
a. Did not attend sites	41.7	38.7	45.1	60.1
b. Attended sites	42.7	40.0	44.6	64.3
Overall	41.1	38.1	43.7	60.7

a/ Each percent is calculated relative to the size of the treatment group in the relevant sample.

rate of job finding is much higher on the follow-ups even though the samples of clients are the same.

Is there any reason to expect errors (other than random) in the self-reported job finding information of the follow-ups? We have already argued that the rate of job finding reported on the 5-97's is likely to be underestimated. It is sometimes believed that welfare recipients, knowing the general expectations of society, may exaggerate the extent of their work effort, leading to an overestimate. On the other hand, men in our sample who reveal work could lose benefits in whole or in part, either through the 100-hour requirement for the control group or through 30-and-1/3 for the other groups. Actually, the desire to conceal work should affect the 5-97's more than the follow-ups, since the forms are official forms for the program. However, to the extent that our questionnaire was mistaken to be an official document, this too could lead to an underestimate. Thus deliberate distortions could produce errors in either direction.

Although we do not have further evidence, we are inclined to believe that the two types of systematic errors in the follow-ups are small. This would mean that the major part of the discrepancy between columns 3 and 4 represents errors in information from the 5-97 forms. If this hypothesis is true, it would suggest that MDES/WIN has relatively poor information on the extent of work effort among clients at any point in time. This in turn would reflect three possibilities: a high rate of concealing work effort among clients; a low level of contact between clients and MDES/WIN offices; or very slow communication between welfare payments and MDES/WIN offices. In the first two cases it could mean overpayments to clients. The discrepancy

between the two sources raises the question as to which is more serious, a work problem among clients or an information problem in MDES/WIN offices.

In terms of data analysis we have two problems: first, that follow-up respondants may not be fully representative of the whole population; second, the measurement of work experience may involve large, systematic errors in either or both of our data sources. As for the non-representativeness of the follow-up, we have already seen that an examination of the 5-97's shows a slight overrepresentation among those submitting follow-up questionnaires of those who find jobs; given the likely errors in 5-97 data, the over-representation could be larger than that suggested in Table V-3. To pursue the matter further, we investigated the relationships between returning the follow-up and a number of other variables. As a crude test, we ran a regression with a dummy dependent variable based on whether the person returned his follow-up. The results appear in Table V-4. They show that fewer non-whites returned questionnaires, but more of those over 40; fewer of those in Experimental Group II who did not attend MASSWEP sites returned them, while more of those who did attend MASSWEP sites returned them. Controlling for other variables, there is not a significant relationship between returning the questionnaire and finding a regular job.

Our main concern in using these data is to identify whether the MASSWEP treatments significantly affected work behavior. To the extent that the non-representativeness of the follow-up sample is related to demographic and treatment variables, this problem can be dealt with by using appropriate control variables. Furthermore, since we have two data sets, each with different statistical problems, we can compare results using each, especially

TABLE V -4

The Probability of Returning the Follow-up: A Regression Analysis

Variable	Coefficient (Standard Error)	Variable Definition	Data Source
Family Size	- .0065 (.0112)		ESARS
Months of Unemployment	.0009 (.0009)	As of date of reappraisal	WIN Barrier Form
Months on Last Job	- .0001 (.0004)		WIN Barrier Form
Age under 25	- .0350 (.0575)	Dummy = 1 if 14 < age < 25, 0 otherwise	ESARS
Age over 40	.0872* (.0415)	Dummy = 1 if age > 40, 0 otherwise	ESARS
High School Graduate	- .0211 (.0417)	Dummy = 1 if years of schooling = 12 0 otherwise	ESARS
Attended College	.0437 (.0598)	Dummy = 1 if years of > 12; 0 otherwise	ESARS
Non-white	- .1524* (.0589)	Dummy = 1 if non-white 0 otherwise	ESARS
Attended WEP site	.1479* (.0450)	Dummy = 1 if assigned to Exper. II and if time card at WEP site shows any hours	Time cards
Experimental Group II	- .0972* (.0422)	Dummy = 1 if assigned to Exper. II, 0 otherwise	WIN Barrier Form
Found Regular Job	.0643 (.0385)	Dummy = 1 if found regular job during 9 month observation period	5-97's
Constant	.5512		

R^2 .04

n 756

F 2.82

* significant at the 5% level

Dependent Variable: Dummy = 1 if Follow-up returned, 0 otherwise

(A dummy for Experimental Group I was tried, but its coefficient was not computed because of too low an F- level.)

tests of whether MASSWEP experimental treatments were statistically significant. However, there are certain practical advantages associated with each data set which limit opportunities for comparison. The follow-up questionnaire contains control variables not available for the whole sample. On the other hand, information on the timing of changes is most accessible from the 5-97's. Of course, it should be remembered that timing information from the 5-97's may reflect more about the rate at which DES/WIN learned about job finding than about the job finding itself. But this is a matter of some interest on its own.

B. The Probability of Finding a Job

In this section we consider the probability that a person in our sample will find a job at some point during the nine-month observation period following the date of his reappraisal; other aspects of employment behavior will be considered in section C. Here we consider only the first job found; if he takes more than one during the period, that will be considered in Section C. We attempt to restrict attention to finding full-time jobs to the extent that the distinction between full and part-time is clear, since in determining eligibility for MASSWEP, both the part-time employed and the unemployed were to be sent to WEP sites.[2]

Finally, in looking at job finding we consider both CETA and unsubsidized jobs together. It is true that policymakers are interested in the two types of job separately. However, our process analysis showed that the men themselves were often confused about the difference, a confusion that seems to have carried over to their responses on the follow-up questionnaire.

[2]The number who found part-time jobs, but never a full-time job is small in any case, amounting to 34 on the follow-up in comparison to 325 who ever found a full-time job.

To the extent that the men cannot distinguish, since it is the behavior of the men we are examining, it may be appropriate to consider the two kinds of jobs together. In any case the number of CETA jobs appears to be small relative to the total. From the 5-97's, where the distinction is clear, only 8.2 percent of the men who never had a regular job had a CETA job. From the follow-ups, 13.6 percent had CETA jobs, although we are not sure if some of these may also at some time have had regular jobs.

In studying job finding, two sorts of disaggregation provide interesting insights, first by time and second by region. From the 5-97's we can obtain a monthly or quarterly record of when men found full-time jobs. Both data sources permit regional disaggregations. Unfortunately, the sample sizes within disaggregated categories are too small to allow meaningful tests of the significance of treatment effects. Following the presentation of disaggregated data, we perform statistical tests to determine significance, but only for whether the person ever found a full-time job during the nine months following his reappraisal.

1. Disaggregated Data on the Rate of Job Finding

a. The Time Pattern of Job Finding

Consider first the timing of finding work as reported on the 5-97's. A 5-97 form is filled out only when there is a change in a person's WIN status. If a person unemployed (or working part-time) has no 5-97 form during a calendar quarter, then we assume that he did not find a full-time job in that quarter. If there is a form during the quarter, then we conclude that a job was found under either of two conditions: if the form indicates a change to working registrant status or if it indicates deregistration--off

AFDC and employed. Since data from the 5-97's permit a clear distinction between CETA and unsubsidized jobs, for the study of timing we examine only the pattern in unsubsidized job finding. All subsequent sections use the combined measure (as done in Table V -3). Finally, if a person finds a job more than once, we consider only the first time. (We considered also monthly transitions but the numbers were quite small; to give a clearer picture we concentrate on first transitions to jobs on a quarterly basis.) Table V -5 presents our results. The quarters are the calendar quarters during which men were observed, ranging from the first quarter of 1978 through the third quarter of 1979. To make comparisons more meaningful, we calculate the percent of men in each group making their first transition each quarter, which is presented in parentheses below the actual number. The base for each percent is the number of men in the treatment group who were in their nine-month observation period for all or part of the quarter and who had not found a full-time job in any previous quarter of their observation period.

A larger percent of men in Experimental Group II found jobs in comparison to Experimental Group I in five of the seven quarters. (This comparison was intended to isolate the effects of the assignment to work sites.) Experimental Group I has a higher percent placed than the Control Group in but three of the seven quarters. (This comparison was intended to isolate the effects of the 30-and-1/3 treatment.) Lastly, a larger percent of men in Experimental Group II found jobs in comparison to the Control Group in six of the seven quarters. (This comparison gets at the combined effect of the two experimental treatments.)

TABLE V -5

Number and Percent Finding First Unsubsidized Job in Quarter[a]

(sample each quarter limited to those in nine month observation period and who have not found job previously)

Year and Quarter	Control	Experimental I	Experimental II	Experimental II		All Groups	Total Sample Size
				Attended Sites	Did Not Attend		
1978 I	6 (9.7)	7 (11.1)	31 (10.0)	3 (3.3)	28 (13.1)	44 (10.3)	429
1978 II	11 (15.9)	9 (13.8)	51 (15.2)	11 (10.8)	40 (17.2)	71 (15.1)	469
1978 III	3 (4.2)	1 (1.5)	38 (10.2)	10 (8.1)	28 (11.2)	42 (8.2)	513
1978 IV	6 (7.8)	6 (8.0)	33 (8.7)	10 (7.5)	23 (9.3)	45 (8.5)	532
1979 I	8 (9.6)	7 (9.6)	43 (11.6)	19 (12.8)	24 (10.7)	58 (11.0)	528
1979 II	7 (10.9)	13 (22.4)	41 (14.1)	23 (19.5)	18 (10.4)	61 (14.8)	413
1979 III	2 (4.4)	0	11 (5.7)	5 (6.7)	6 (5.1)	13 (4.7)	276
All Quarters	43 (28.7)	43 (30.7)	248 (34.2)	81 (31.6)	167 (35.6)	334 (32.9)	
Total Sample Size	150	140	725	256	469		1015

a/ Record of job finding taken from 5-97 forms.

Even though there is consistency in two of the three above patterns, the quantitative differences generally are small. It remains to be established whether they are significant. It also is necessary to recall from Chapter IV two points that are critical in interpreting the observed patterns: during the first half or so of MASSWEP, the 30-and-1/3 treatment barely was implemented in Experimental Group I; thus it should not be surprising to find no consistent effect in comparing Experimental Group I and the Control Group during the first four quarters. Secondly, during the later months of MASSWEP, getting men in Experimental Group II to work sites may have been emphasized less than getting them regular jobs.

A more interesting picture emerges when Experimental Group II is broken down between those who did and did not attend MASSWEP sites. Among those who did not attend sites, the rate of finding jobs is noticeably greater than among those in Experimental Group I and the Control Group for the first five quarters. Considering possible explanations, one is that the higher rate of job finding may reflect pressure from MASSWEP early in the program which eventually diminished. The pressure could take the form of direct urging or renewed assistance by WIN counselors to get the client into a job quickly, or it could come from the client's desire to avoid going to a MASSWEP work site. A second possibility is that the differences show not a higher rate of job finding but a higher rate of WIN staff

learning about existing client jobs. Our interviews with WIN workers suggested that it was common for clients fearful of losing welfare benefits to hold jobs without informing their WIN office. Assigning a client to a work site is a way to smoke out concealed jobs. Under this hypothesis, the change in records would come shortly after the client's reappraisal, consistent with the observed time pattern of an early effect which diminishes with time.

The record of those who went to MASSWEP work sites provides a contrast. Among these the rate of job finding is substantially lower than for those who never went to sites for the first two quarters of 1978, but eventually becomes higher than the rates for all other groups in the first three quarters of 1979. If men spend time at work sites, it might be expected that they would not find regular jobs right away. On the other hand, if they do benefit from their 13 weeks (one quarter) at a work site, they should find a regular job shortly after that. The time pattern for this group does show a lagged effect on the rate of job finding, but the lag is too long for the simple hypothesis. By the first quarter of 1979 when the increased job finding appears, almost half the sample had completed their nine-month observation period. If the MASSWEP work experience were to have a meaningful effect, it should show up relatively soon after leaving the work site--within one quarter or maybe two. Of course, the first year of the program was disrupted by the injunction, but this ended by May. The third quarter of 1978 does show a higher percent of job finders among clients who attended sites than in the Control Group, but the rate of activity in all groups is very low. The fourth quarter shows no effect. It is only

in 1979 that a differential effect appears to emerge--and then in only two of the three quarters, when comparing those attending sites with those in the Control Group. But this corresponds to the period when the state began to tighten the administration of the program.

Does this imply that the work site treatment succeeded in getting men into regular jobs: first, it has not been established yet that the higher rate of job finding for this group is statistically significant. Second, men were going to MASSWEP work sites in 1978 without apparent effect. It may well be that if a significant difference exists, it results not from the fact that these men attended work sites, but from the extra efforts and assistance they began to receive from WIN counselors under pressure from the state.

To summarize, the differences in the rate of job finding between groups in Table V -3 are consistent with the idea that possible and actual assignments to work sites has a deterrent effect: those in Experimental Group II not attending sites did either find or report jobs at a higher rate during the early phase of the experiment than did those in Experimental Group I. On the other hand, for those sent to work sites, a difference appears only a year into the experiment, and then not consistently. It is likely that this effect results not from the benefits of attending work sites, but from extra attention and assistance these men received during this limited period of concentrated state effort.

Table V -5 shows also the rates of job finding when all quarters are aggregated together; in other words, the rate at which men found a job

at some point during their nine-month observation period. For the whole period, both parts of Experimental Group II find jobs at a higher rate than either Experimental Group I or the Control Group. However, the more pronounced effect during limited sub-periods is no longer visible, especially when comparing those attending sites with those in Experimental Group I.

It remains to be established whether the observed differences are statistically significant. We ran seven quarterly regressions, each with a dummy dependent variable indicating whether the individual found a job in that quarter. Independent variables included controls for a number of demographic and economic characteristics as well as program treatment variables. Unfortunately, none of the regressions taken as a whole had significant F ratios. Three of the seven regressions had no significant coefficients. The remaining four each had one significant coefficient, but a different one in each case. No treatment variable was ever significant. From these results, we concluded that we could not proceed further with statistical analyses of the data broken down by time periods. All remaining statistical tests of job finding involve the whole nine month observation period for each client.

b. The Regional Pattern of Job Finding

Table V -6 presents the rate of job finding (unsubsidized and CETA combined) over the nine-month observation period, but disaggregated by region. Both data sources are used; the 5-97 data are presented for the whole sample as well as for those who returned follow-up questionnaires. It should be noted first that sample sizes within

TABLE V -6

Percent Who Found Jobs, Broken Down by Region

a. Whole Sample of 1015 (5-97 data)

Treatment Group	Northeast	Western	Central	Metro	Southeast	Boston
Control Group	48%	29	33	33	48	32
Experimental I	48	31	43	29	39	30
Experimental II	43	29	52	43	52	29
Did not attend MASSWEP sites	45	27	48	42	55	28
Attended MASSWEP sites	37	32	58	44	46	32

b. Follow-up Sample of 535 (Follow-up data)

Treatment Group	Northeast	Western	Central	Metro	Southeast	Boston
Control Group	78%	56	50	71	31	46
Experimental I	36	70	54	50	65	56
Experimental II	59	51	71	66	70	55
Did not attend MASSWEP sites	62	50	68	52	79	63
Attended MASSWEP sites	52	52	75	80	55	46

c. Follow-up Sample of 535 (5-97 data)

Treatment Group	Northeast	Western	Central	Metro	Southeast	Boston
Control Group	72%	33	38	35	38	31
Experimental I	36	50	38	38	39	33
Experimental II	40	33	55	44	52	41
Did not attend MASSWEP sites	42	29	53	40	58	40
Attended MASSWEP sites	37	38	58	48	42	43

ADDENDUM TO TABLE V -6

Follow-up Sample Sizes Within Cells

Treatment Group	Northeast	Western	Central	Metro	Southeast	Boston	Total
Control Group	18	9	16	17	13	13	86
Experimental I	14	10	13	8	23	9	77
Experimental II	69	49	62	50	79	63	372
Did not attend MASSWEP sites	50	28	38	24	48	35	224
Attended MASSWEP sites	19	21	24	25	31	28	148

treatment groups by region are very small. The Addendum to Table V-6 presents these for the follow-up sample. For the central region there appears to be a substantial difference between Experimental Group II and the other two treatment groups in the expected directions. The same difference appears in all three parts of Table V-6. For the metro and southeast regions, similar differences appear, but only in some parts of the table, not all three. The evidence is weak but is consistent with the argument in the process analysis that the program was pursued more vigorously in some offices than others. If this is a valid result, it suggests that a work experience program, vigorously administered, could increase the rate of job finding among WIN clients. The question then arises as to whether such a program could be administered vigorously everywhere, or whether the uneven pattern observed in MASSWEP with the weak overall effect on job finding is the likely general situation.

2. Statistical Tests

In view of the difficulties in using disaggregated data, we concentrate in this section on the probability of finding work over the whole nine-month observation period for each individual. To study job finding, we construct a dependent variable which equals one if the person ever found a job during his nine-month observation period, zero if he did not. We have this information from both the 5-97 forms and the follow-up questionnaires, so that we construct two job finding variables to investigate each data source separately. We want to relate the job finding variables to a series of treatment and

control variables. We shall discuss first the choice of variables, then the estimation technique, and finally the results.

a. Independent Variables

The experiment was designed to provide convenient measures of experimental treatment. We constructed one dummy variable to indicate whether the individual was in Experimental Group I and a second for those in Experimental Group II. The process analysis emphasized that actual treatments to individuals in both groups were uneven. For example, individuals in both experimental groups were to be eligible for 30-and-1/3, but were often not informed of this in a comprehensible way. Most variations in actual treatment are not readily measurable so, for the most part, we rely on assigned treatment group. One type of variation we did measure relates to the fact that although all in Experimental Group II were eligible to be sent to MASSWEP sites, only some actually went. We constructed a dummy variable indicating whether the individual actually went to a MASSWEP site.

In order to identify the pure effect of MASSWEP treatments, we introduced a number of variables to control for other factors that might affect job finding. These include a number of demographic factors like race, family size, age, and education, where the latter two are broken into categories represented by dummy variables. These variables are taken from the ESARS forms of MDES/WIN.

Standard theory suggests that a number of economic variables will affect the probability of finding a job. On the supply side, variables like wage rate and income are expected to affect the willingness of a

person to search for and accept a job. We have two possible sources for income, both very poor. First, the ESARS forms, filled out when the person first entered WIN, report income over the previous 12 months. However, the definition of income used is unclear. Moreover, income at the point of going on welfare or into WIN may reflect special circumstances which no longer hold by the date of reappraisal. Indeed, income is either zero or not reported (we cannot distinguish which) on 892 of the 1092 ESARS forms we collected. The second source is our follow-up questionnaires. However, the date of this information is the end of the nine-month observation period, after the person may have gone to work. We are unable to distinguish earnings from other income so that this measure would not be exogenous or predetermined; using it would introduce a risk of bias. In view of the imperfections in data, we did not use an income variable. Our difficulties may indeed reflect a more general problem in obtaining income measures for a welfare population: unearned income is likely to undergo frequent shifts as people move on an off welfare, so that it is difficult to decide which figure will have the most important effect on behavior.

Wage rate data were more usable than work data. Wage data from the follow-up were only for those who had found jobs and thus not useful. However, the work history forms of MDES/WIN included wages received on past jobs. Of the 991 work history forms we received, the wage on the most recent job was missing in 175 cases; on the second most recent job in 428 cases; and on the third most recent job in 672 cases. In addition, the wage information is in some cases quite old. Nevertheless, it was decided to

use a wage variable, but only that for the most recent job to minimize the number of missing cases.

While wage and income are often associated with long-run supply, the men in this sample may be having short-run troubles in the labor market not reflected in a long-run supply curve. We use two variables to control for short-term instability:[3] the number of months unemployed (as of the date of reappraisal) and the duration (in months) of the most recent job, both taken from the WIN barrier form. Other measures of job instability were tried, using information about the second and third most recent jobs. These variables were never significant. In view of their arbitrary construction, they were not used in the end.

In addition to variables measuring the extent of instability, we tried a number of variables related to reasons for instability. The follow-up asked if the person's health was good, average, or poor. We constructed one dummy variable indicating whether the person answered "poor" and a second if he answered "average." In addition we constructed a number of variables based on barriers to employment indicated on the WIN and SAU barrier forms. None of the barrier variables was ever significant; in the end we decided not to use them.

b. Estimation Techniques

The principal problem is that the dependent variable is a zero-one dummy variable. Regression is generally believed to be unbiased and consistent, but not efficient with such a dependent variable because of

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[3] We use the term short-run to distinguish from the long-run supply curve, but we are referring to unemployment situations which may have lasted a long time.

heteroscedasticity. Logit is often considered better in principle, but difficult to estimate. For the job finding variable based on follow-up data, we decided to use both regression and the approximation approach to logit included in the SAS package. The regression results with the 5-97 data were so poor that we did not try the logit.

Letting Y_i be the dummy dependent variable, ordinary least squares regresses it directly on the independent variables. In logit, the dependent variable is transformed before estimation. The magnitudes of the coefficients may not be comparable. Indeed, the effect of a given change in an independent variable on Y_i will vary under logit although it is constant under ordinary regression. (Under logit the effect on the transformation of Y_i is constant.) While not comparing magnitudes of coefficients, it is possible to compare signs and significance. We use t tests for the significance of coefficients using ordinary regression and chi square tests for the coefficients of logit.

c. Results

Table V -7 presents the results using both ordinary regression and logit for the dependent variable drawn from the follow-up data and ordinary regression for the 5-97 data. Testing the overall significance of the regressions, the F-ratio using follow-up data is significant at the 5 percent level while it is not using the 5-97 data. Moreover no individual coefficient is significant using the 5-97 data. Therefore, we base the discussion on the tests using follow-up data. Even though the regression in column 1 is significant, its R^2 is small. The small R^2 is inherent in the use of a dummy dependent variable which takes on values of only zero or one,

TABLE V-7

Coefficient Estimates for the Model for the Probability of Finding a Job

Data Sources for Dependent Variable	(1) Follow-ups	(2) Follow-ups	(3) 5-97's
Estimation Technique	Regression	Logit	Regression
Independent Variables			
Family Size	.0035	.0189	-.0147
	(.0169)	(.0834)	(.0185)
Age Under 25	.0609	.3355	.0097
	(.0862)	(.4560)	(.0944)
Age Over 40	.0104	.0535	.0450
	(.0574)	(.2843)	(.0629)
High School Graduate	.1097	.5937	
	(.0629)	(.3337)	
Attended College	.05866	.2961	-.1112
	(.0894)	(.4615)	(.0958)
Non-white	-.2028*	-.9604*	-.1767
	(.0951)	(.4596)	(.1039)
Wage	.0113	.0630	.0200
	(.0123)	(.0639)	(.0129)
Months of Unemployment	-.0025*	-.0115	-.0023
	(.0012)	(.0061)	(.0014)
Months Last Job	-.0001	-.0009	-.0006
	(.0006)	(.0029)	(.0006)
Poor Health	-.5014*	-2.3930*	-.1833
	(.0920)	(.5041)	(.1008)
Average Health	-.1587*	-.7541*	-.0604
	(.0581)	(.2805)	(.0632)
Attended MASSWEP site	-.0657	-.3455	-.0670
	(.0621)	(.3124)	(.0680)
Experimental I		-.0143	-.0877
		(.4590)	(.1006)
Experimental II	.0358	.1848	-.0170
	(.0607)	(.3790)	(.0832)
Constant	.703	.8566	.5995
R^2	.1523	.171[a]	.0552
n[b]	323	323	323
F	4.27	50.78[c]	1.39

Variable Definitions

Dependent Variable: dummy variable = 1 if found work during nine months following reappraisal, 0 otherwise.

Wage: hourly wage on most recent job before reappraisal, from work history form.

Poor Health and Average Health: dummy variables constructed from responses on question which asked whether health was good, average, or poor, from follow-up.

Footnotes

(a) For logit, predictive accuracy coefficient reported instead of R^2.
(b) All cases were excluded from the estimation for which there was any missing information.
(c) For logit, model chi square reported instead of F.
* Indicates significance at the 5 percent level; established by t tests for regression, chi square for logit.
(Standard errors reported in parentheses below each coefficient.)

while values of Y_i predicted by the regression will take on a range of values, seldom equal to zero or one. Our logit program estimates a predictive accuracy coefficient of .17.

Now consider the coefficient estimates. For the follow-up data, all logit and regression coefficients agree in sign. In both equations the strongest variable is poor health, having the expected negative sign. Average health is also significant, also with a negative sign (the comparison is to good health). The race coefficient is significant with a negative value, suggesting a lower probability that Non-whites will find jobs. The only other significant variable is the length of time unemployed. It is significant at the 5 percent level only for the ordinary regression estimate but it is close to the 5 percent level in the logit equation. Its negative sign is consistent with expectations. It is an interesting variable in that its significance would indicate that men can get locked into unemployment if they remain unemployed long enough: the longer they are unemployed, the lower the probability of finding work. Our results weakly support this hypothesis, but not definitively. To summarize our significant coefficients, the probability of finding a job is reduced by both poor and average health, by being Non-white, and possibly by having been long unemployed.

It is noteworthy that neither of the experimental treatment variables is significant at all. Indeed using stepwise regression the tolerance level for Experimental Group I was too low for it to be included in the follow-up equation and similarly for Experimental Group II in the 5-97 equation. Controlling for many other variables, we thus found no significant effect from either experimental treatment. We ran the regressions using treatment

variables alone without the control variables and again found no significance to the treatment variables. The insignificance in the complete equations thus cannot be attributed to a high collinearity with control variables which hide the true effect of the treatment variables.

As a further test, we ran the regressions separately for each of the three treatment groups using follow-up data, with the results appearing in Table V -8. Introducing treatment only as dummy variables as in Table V-7 restricts the effect of treatment to shifting the constant term alone, while separating the regressions for the treatment groups allows more complex interactions between treatment and other variables to appear. Sample sizes become small when the treatment groups are analyzed separately. Since there are many missing observations on the wage variable and since wage was completely insignificant for the whole group using follow-up data, we omitted it from the treatment group regressions, gaining a few degrees of freedom. Even so, the regressions for the Control Group and Experimental Group I, taken as a whole are not significant. Moreover, no individual coefficients are significant in the Control Group regression. Nevertheless, it is interesting to compare coefficients across groups. For the variables that are significant for Experimental Group II--Non-white, months of unemployment, poor health, and average health--coefficients agree in sign across all three groups and are even close in magnitude, especially between Experimental Group II and the Control Group. There is thus no indication that receiving an experimental treatment affects the rate of job finding or the influence of explanatory variables on this rate. In short our results on job finding, using both data sets, both estimation techniques, as well as the separation

TABLE V-8

Probability of Finding A Job: Ordinary Regression Coefficients for Each Treatment Group Run Separately

Sample	Control Group	Experimental I	Experimental II
Data Source for Dependent Variable	Follow-ups	Follow-ups	Follow-ups
Family Size	.0121 (.0368)	-.0192 (.0403)	.0103 (.0170)
Age Under 25	.0195 (.1720)		.0185 (.0929)
Age Over 40	-.0769 (.1416)	.1688 (.1430)	
High School Graduate	.2313 (.1432)	.1068 (.1434)	.0949 (.0639)
Attended College		.0756 (.2251)	.1495 (.0874)
Non-white	.2315 (.2273)	-.3679 (.2380)	-.2196* (.1024)
Months of Unemployment	-.0035 (.0026)	-.0015 (.0030)	-.0031* (.0013)
Months on Last Job	-.0011 (.0009)	.0020 (.0015)	
Poor Health	-.3568 (.1898)	-.8267* (.2827)	-.4231* (.0964)
Average Health	.1465 (.1287)	-.2798* (.1368)	-.1981* (.0620)
Attended MASSWEP Site			-.0596 (.0557)
Constant	.6782	.7578	.7706
R^2	.2333	.2651	.1580
n	65	61	275
F	1.86	2.044	5.52

Dependent Variable: Dummy Variable indicating whether the person found a job based on follow-up data.

* Indicates significance at the 5 percent level.

by treatment groups, show no statistical significance of being in either of the treatment groups in the MASSWEP program.

3. Interpretation

Our discussion so far has concentrated on one central question: whether the treatments in the MASSWEP program had an effect on the probability of men finding jobs. To investigate this question we relied on the experimental design of the study in which clients had been assigned randomly to the two experimental treatment groups and the control group. The raw data on job finding, when disaggregated by time and by region, suggest some possibility that the WEP treatment mattered. However, statistical tests found no significant differences in the rate of job finding between treatment groups. The results in the process analysis raised serious questions about how thoroughly the MASSWEP treatments were administered. The possibility remains that a similar program, differently administered, might have worked, although similar problems might arise again in administering such a program. What we can conclude with a high degree of confidence is that the program actually operated in Massachusetts had no statistically significant effect so that if it had an effect at all, it must have been too small for our analytical tools to detect.

There is a second question to which our evidence is relevant, relating to the characteristics of the men in the program and their needs. The MASSWEP program was restricted to men who had been unemployed for at least six months. It was assumed that such men were relatively hard core unemployed who would need either assistance or pressure to get them back to work. MASSWEP was

intended to be that combination of assistance and pressure that would deal with this special hard core population within the WIN unassigned recipient pool. Our results suggest that not only was MASSWEP not the success intended, but the analysis of the needs and problems of the target population was faulty. In particular our results question whether the men in the program can be considered hard core unemployed. Clearly, although they experience long-term unemployment, they are not unemployable.

There is by now an extensive accumulation of evidence on how people mix work and welfare. Our results at least partly confirm the same tendency. Even though all men in our sample had been unemployed for at least six months before their reappraisals, large numbers of them found jobs during the next nine months. Relying on follow-up data, Table V-3 showed that 57 percent of those in the Control Group found jobs and 56 percent of those in Experimental Group I. For Experimental Group II it is 63 percent, higher but not significantly so. Thus even among those who received no treatment under MASSWEP, at least 55 percent found regular jobs. This is not to say that the jobs were good or lasted long--subjects to be investigated in Section D. However, a majority of men typical of our sample found jobs. They took considerably longer than average to find jobs, but they are not unemployable. They fit the work and welfare hypothesis even though it may take them some time to find jobs. The lack of significance to the MASSWEP treatment suggests that it is basically not successful in increasing the amount of work already taking place.

While over 55 percent of the men found work on their own, the remainder did not during their nine-month observation periods. Do these men constitute hard core unemployed or unemployables? It is conceivable that even some of

these would have a non-zero probability of finding a job, given more time. However, this group probably can be considered largely unemployable--men who over a very long period do not work. Our results are thus consistent with the hypothesis that there is a hard core of unemployables, but six months of unemployment do not establish a meaningful dividing line: within the six month group, the majority are work and welfare mixers rather than permanent welfare dependents.

Given that our population contains both men who are and are not hard core unemployables, is is possible to distinguish between them? The regression results of Table V-7 provide evidence. The question is not easily made operational. Our regression or logit models predict for each client a probability of finding a job which may range continuously from zero to one. To define unemployability precisely, it would be necessary to designate a critical level such that any predicted probability below it could be considered hard core unemployment. Clearly, any such critical level would be arbitrary. Our model allows us to identify a tendency toward being unemployable rather than a firm designation.

What then contributes to the tendency to be unemployable? First, health is a major factor in distinguishing between job finders and others. However, some caution is needed in interpreting this result. The health variable is a self-reported one from the follow-up questionnaire. Indeed, it may be in part a work attitude variable: a person who really does not want to work may excuse that desire by finding a health problem for himself. However, whether the self-reported health problems are real or not is immaterial in a sense. The health variable is an important predictor of the probability of finding a job.

Second, the length of time unemployed--if it is really a significant variable--supports the idea of a hard core of unemployables. If the probability of finding a job declines steadily with the length of time unemployed, a person will eventually tend to become locked into the state of unemployment. However, our results show that the point of being locked in is well beyond six months: at six months the probability of finding a job is still reasonably high. Third, race is significantly related to the probability of finding a job.

Suppose one wanted a formula to predict which clients will be hard core unemployables--or even which have a high probability of becoming hard core (a low probability of finding a job). Our results can provide an estimate of the probability of finding a job for each client by using the estimated coefficients to predict a value of the dependent variable. However, these estimated probabilities will be quite imprecise as indicated by the low R^2 in the regression (15 percent using follow-up data) or the predictive accuracy coefficient of 17 percent in the logit. One could have tried a longer list of independent variables. We used those available to us which would have some degree of reliability. It is our impression that even an expanded list of measurable independent variables would contribute only marginally to the predictive accuracy of our model. While on the average there are a few characteristics which the relatively hard core share, this is not information that can translate into a reliable evaluation on a client by client basis. The subjective valuations of the WIN worker are essential, alhtough even these may err.

To conclude, we have found many in our sample who mix work and welfare and are definitely not hard core unemployables. There are others

who might well be, at least in the sense of having a relatively low probability of finding jobs. Furthermore, there are a few variables systematically related to the probability of finding a job. MASSWEP was a program designed to treat both groups, but the treatment appears to have had little detectable effect on men of either category.

C. Experiences of Job Finders and of Non-Job-Finders

This section considers in more detail both job finders and those who never found jobs. Had section B found evidence that the experimental treatment significantly affected the rate of job finding, then it would be particularly important to observe the nature of the jobs found. It is possible that a program which pressures people into accepting jobs succeeds by putting people in less attractive jobs which they are unlikely to keep. However, since we found no significant treatment effect on job finding, the purpose of this section is simply to check if there might be some subtle effect of the program which does not show up in job finding itself.

Table V-9 summarizes the experiences of job finders. Line 2 repeats information on the rate of job finding from Table V-3. Lines 3 and 4 report the wages on the first job found, 3 the mean and 4 the median for each group. As might be expected, the median is always below the mean. However differences between groups do not appear large using either measure. The differences between jobs in hours worked per week are also small.

One of the interesting questions relates to how long the person kept a newly found job. We are limited in observing this, since we followed each person for only nine months following reappraisal. Given that many jobs were not found immediately, it is not surprising that nearly two-thirds of those in all groups still held the first post reappraisal job they found (line 6). For

TABLE V-9

Experiences of Job Finders

	Control Group	Experimental I	Experimental II	
			Attended WEP Sites	Did Not Attend WEP Sites
1. Follow-up Sample Size	86	77	148	224
2a. Number Who Ever Found Jobs	49	43	89	144
2b. Percent (2)÷(1)x100	57.0%	55.8%	60.1%	64.3%
3. Mean Wage on First Post Re-Appraisal Job	$4.81	$4.90	$4.72	$5.37
4. Median Wage	$4.17	$4.00	$4.27	$4.35
5. Mean Weekly Hours Worked	35.3	34.5	37.0	37.3
6a. Number Still Working at First Post Reappraisal Job	31	28	58	89
6b. Percent of all Job Finders (6a)÷(2a)x100	63%	65%	65%	62%
7a. Number Obtaining Second Post Re-Appraisal of all Job Finders	6	5	9	30
7b. Percent of Those Who Left Their First Job (7a)÷[(2a)-(6a)]x100	33%	33%	29%	54%
7c. Percent of All Job Finders (7a)÷(2a)x100	12%	12%	10%	21%

those who had left their first job, the median duration was just below five months (not shown in the table). Of those who left the first job, about one-third obtained a second, except among those in Experimental Group II who did not attend WEP sites where the fraction is over half (line 7b). In terms of the whole job finding population, this amounts to about 12 percent of second job finders. The numbers at this stage are so small that little significance can be attached to the difference between groups.

As one further piece of information about job finders, we examined the nature of the jobs found. Even using the one digit categories of the Dictionary of Occupational Titles, the number of cases per category after a breakdown into treatment groups is too small to make meaningful comparisons. However, for all groups together we did look at job types and transitions from pre- to post-reappraisal jobs. Table V-10 gives pre-reappraisal job categories vertically and post-reappraisal categories horizontally. Each cell shows the number making the indicated transitions and in parentheses that number as a percent of those in the same pre-reappraisal job category. Clients in our sample tend to concentrate in categories 2 (clerical and sales), 3 (services), 6 (machine trades), 8 (structural work), and 9 (miscellaneous). Except for structural work occupations, the majority of those in any category before reappraisal end up in a different category afterwards. If there is any overall pattern to the movements between categories, it appears that men are moving into service and structural occupations, but the changes are not large. In general our examination of job experiences does not provide new evidence of strong effects from the experimental treatments.

Turning now to those who were not working we asked such men the reasons on the follow-up questionnaire. Although our regressions for the

TABLE V-10

Pre- to Post-Reappraisal Job Category Transitions

Previous Job \ First Post-Reappraisal Job	Clerical And Sales	Services	Machine Trades	Structural Work	Miscellaneous	Other	Total (Pre-Appraisal Percentage
Clerical & Sales (Code 2)	7 (20)	8 (23)	4 (11)	7 (20)	4 (11)	5 (14)	35 (11)
Service Occupations(Code 3)	2 (5)	14 (37)	0	12 (32)	5 (13)	5 (13)	38 (12)
Machine Trades (Code 6)	1 (3)	6 (18)	8 (24)	7 (21)	7 (21)	5 (15)	34 (11)
Structural Work (Code 8)	2 (3)	8 (12)	3 (5)	40 (62)	8 (12)	4 (6)	65 (21)
Miscellaneous (Code 9)	6 (9)	12 (18)	2 (3)	12 (18)	24 (36)	11 (16)	67 (22)
Other (Codes 0,1,4,5,7)	8 (12)	12 (18)	3 (4)	11 (16)	16 (24)	18 (26)	68 (22)
Total	26	60	20	89	64	48	307
(Post Reappraisal) Percentages	(9)	(20)	(7)	(29)	(21)	(16)	

Parentheses give percents of those in the same previous job category.

Codes refer to Dictionary of Occupational Titles Categories

Previous job categories obtained from MDES/WIN Work History forms, post reappraisal categories from follow-up questionnaires.

probability of finding a job identified few variables related to finding jobs, our purpose on the follow-up was to obtain more detailed information by asking men directly their reasons for not working. They could check more than one of the listed responses. The numbers of responses to individual reasons within treatment groups were too small to make meaningful comparisons. Therefore, Table V-11 presents the reasons for not working for all groups combined.

TABLE V-11

Reasons for Not Working

	a. Never worked		b. Had worked, but were currently unemployed	
	Number of Responses	Percent of Non-workers (210)	Number of Responses	Percent of Respondents (79)
Long term illness or disability	38	18.1	4	5.1
Accident or temporary illness	18	8.6	5	6.3
Taking care of sick person at home	8	3.8	1	1.3
Taking care of children at home	15	7.1	1	1.3
On strike	0	0	0	0
Laid off and awaiting recall	13	6.2	19	24.1
In school	4	1.9	1	1.3
In job training program	7	3.3	1	1.3
Searched, but could not find job	127	60.5	33	41.8
Feared losing welfare benefits	18	8.6	7	8.9
Other	33	15.6	30	38.0

These results confirm that there are long-term health problems--at least by self perception--among 18 percent of those who never worked. However, the most common reason was not any such formal obstacle, but rather the inability to find a job, given by 60 percent of never-workers. The regression dependent variable from section B separated those who had never worked during their nine-month observation period from those who at some time had. Among those who had, some were unemployed when they returned the follow-up questionnaire. Part b of the table presents the reasons for these men. In comparing the two groups, it is noteworthy that being unable to find a job is still the most common reason among sometime-workers, but now given by only 40 percent. The number awaiting recall is much higher.

Returning to the question of how many men in the program are hard core unemployables, we are confronted with the 60 percent of never-workers who searched, but could not find jobs. For some of these search may really be fruitless while for others, they may still find work if they look long enough. However, our data do not allow us to probe deeper into the question of why these men are not finding jobs. Our results are at least consistent with the existence of a hard core group. In addition to these are those with long-term illnesses who possibly were not screened properly. There are clearly many in the sample with significant work problems and for whom the MASSWEP program made no difference.

D. Client Perceptions of the Effects of MASSWEP

Experiences at work sites may have had effects that were not detected by the foregoing statistical analysis. This section explores this possibility by examining the comments made by participants about the consequences of their work assignment. WIN clients who were part of the MASSWEP experiment had the opportunity to express their feelings about the program in the work site personal interview and in the mailed follow-up questionnaire. In Chapter IV, we touched upon some of the positive and negative reactions of men who went to work sites. Before turning to the follow-up questionnaires, we will look further at the work site interview. The section concludes with an attempt to evaluate the impact of attitudes on work effort statistically.

1. Work Site Experience

General reactions to work site experience itself were mixed: the experience itself typically was not unpleasant, but clients often noted that it did little to enhance skills. Men were asked to rate the program on a scale ranging from 1 (bad) through 10 (good). Approximately 24 percent rated the program at each of the extremes with the next largest group at the scale's mid-point. No man who went to a cemetery as a work site had a positive comment, and while cemeteries served as sites throughout the state, few men spent as much as two or three days on these sites. In the later part of the program, some Boston teams reappraised men in groups and assigned them to the site in groups. One man reported that

the group going to the cemetery was told that each person would work at what suited his background: he expected to drive a truck; another man arrived wearing a shirt and tie expecting to work in the office. All of them were told to rake leaves, in spite of the rain. One older man said he had to walk two miles and there was no public transportation:

> "I got sick--I never did outdoor work before; I worked in a factory all my life."

Another noted that:

> "CETA kids were getting $169.00 a week and smoking pot while we were getting $3.00 per day. That's stupid," he concluded.

Positive work site experience in terms of training, rested on a good match between skills and sponsor needs, but such a match was not common. Many men thought the work site assignment was or would develop into a WIN OJT job or a CETA job. They learned that it did not. Others, though, believed that the stipend and travel expenses were additional income for "work" and would go on indefinitely. One "bored" site attendee said the program should "pay the people more money to work on WIN. Also, it should be for a 40 hour week instead of three days."

2. Perception of Effects of Work Arrangements

The men in MASSWEP jobs were queried about whether they had benefited from it. Each of the mailed follow-up questionnaires asked as a final question, "Do you have any other comments, good or bad, that you would like to make about the Work Experience Program?" The responses reflect this heterogeneous group with diverse levels of education and a wide range of work histories. Responses regarding effects ran the gamut, from those indicating that work assignments aided the individual in making a transition

to the regular labor market, to those suggesting that MASSWEP had been disruptive.

Consider remarks indicating that work assignments were positively assistive:

> "At first I was upset to think that I was working for about $1.00 a day but after taking into consideration the AFDC check I received, I realized it was much more. It also got me out of the house and back into the working field. They sent me on an interview and I got the job I am on now."
>
> "It gave me a sense of responsibility and a regular routine."
>
> "It was a good program because it gave me the experience I need to get a steady job. I got a good work record out of it."
>
> "They did good by me. They found me the first job at the steel company."

Some quite pointed comments reflected the view that the program was of no value or, worse yet, costly because it was inappropriately targeted:

> "Make it apply to people who really have nothing to offer the job market--don't waste time and money on people who are trained, but in between jobs, even though sometimes the wait for work can be extensive."
>
> "I think it's a hell of a program for those who don't have any trade or any smarts. They can get job training to learn a trade and get back into the job market. For me it was no good because I've got a trade and a union. I expect to be back to work in the next six months. I wouldn't want to lose all my benefits now."
>
> "I believe that any normal person with self confidence and willing to seek work of a trade he/or she is accustomed to and which pays income necessary for his family's personal needs of survival should not have his rights to normal routes of seeking employment taken away--with harassment to degrade him into forced labor at unfamiliar work at incomes far below what he needs to survive."

Here are some other comments from men who went on sites:

"If there won't be a job at the end of the program, it is a waist (sic) of time."

"WIN worker very concerned about finding me a job."

"They give you a job that don't require any experience. Anyone can take a mop in their hands. They told me they sent me to work on a site just to get me out of bed in the morning. I don't see why they can't get me a regular job at a decent wage."

"They need social workers who would go to the place of work and find out the work conditions of some places where they send people."

"You felt like a piece of shit working there. Everyone knew you were on welfare because of what you are doing and they looked down their nose at you."

Several of the men who were asked to go to sites and men who actually went complained about the cost of getting there. This was summed up by an Experimental II who didn't go to a site.

"I think the program stinks. They pick a work site that is 40 or 50 miles away and it's impossible to get there unless you have a car. Then it takes more money than what they give you to run your car."

Two men in Experimental Group II who didn't go to a site praised the staff and WEP:

"It gave me the opportunity to learn how to look for a job."

"They treated me well. They helped me to get a job by showing me how to approach companies and ask for work. They helped me to get my confidence back."

3. The Impact of Attitudes on Job Finding

This section reports on our effort to link work site-related satisfactions as well as general work orientations to job getting in the nine month period after initial MASSWEP reappraisals. Detailed information about satisfactions and work orientation is available only from the personal interviews conducted at work sites. The sample is thus limited to those in Experimental Group II who attended work sites. This section thus cannot evaluate experimental treatment controlling for attitudes. It can simply provide an indication of whether attitudes make a difference in job finding for this group.

The dependent variable used in the regression analysis is whether the father who attended a site ever got a job in the nine months post-reappraisal as measured by the Brandeis follow-up questionnaire. The independent variables include many of the control background variables identical to those in Table V-7. In addition there is a summary index based on sources of MASSWEP work site satisfaction. Factor analysis of all items was undertaken, scales formed, and scale reliabilities tested. A similar process was undertaken for two scales formed from single items that measure general work orientations. The original work orientation items came from several previous studies concerning the work ethic of low-income welfare recipients.[4] Regressions were run first using the MASSWEP work satisfaction variables and then separately the work orientation variables.

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[4] These and related studies are reviewed in detail in Hahn, Andrew B. The Voluntary Dimension in Work Effort: The Effects of Work and Leisure Orientations on the Work Effort of Low Income Family Heads. Dissertation: Heller School, Brandeis University Microfilms, Ann Arbor, MI 1978.

Table V-12 reveals that among men who had attended a work site, previous employment history, age, ethnicity, and health status all predict significantly the likelihood of finding at least one job in the nine month study period. However, neither work site satisfactions nor general work orientations are significant predictors. Results from unreported analyses employing a variety of other social-psychological variables from the personal interviews of men on sites similarly found these variables insignificant. Some men may have enjoyed the work site experience while others may have detested it; our measures, however, detected no effect of such feelings on the probability of finding a job over a nine month period.

The absence of effect may mean either that work satisfaction and/or work orientations were inadequately measured, or that they really did not matter. Part of the justification for MASSWEP was that recent work experience would assist the long-term unemployed to find unsubsidized jobs. In part they would benefit from a new entry on their resume, but they also might internalize some features of the work experience in a way that would help them find a job. If there is no effect of work satisfaction or work orientation, then internalizing features of the work experience would be unlikely to affect the chances of finding an unsubsidized job.

Table V-12

The Effect of Work Site Satisfaction and General Work Orientations on Getting a Job in the Nine Month Period After Reappraisal (Sample: Men Who Attended Work Experience Sites)

Family Size	.338 (.029)	.434 (.031)
Length of Unemployment	-.537* (.002)	-.554* (.002)
Age-Young	.362* (.161)	.405* (.168)
High School Graduate	.130 (.115)	.147 (.120)
Attended College	.169 (.135)	.190 (.145)
Non-White	-.459* (.209)	-.445* (.210)
Poor Health	-.572* (.168)	-.576* (.165)
Average Health	-.792 (.110)	-.958 (.113)
MASSWEP Satisfaction Index	-.636 (.055	
Fate Control		.129 (.031)
Work Ethic		.203 (.063)
Constant	.606	.424
R^2	.353	.362
N	88	86
F	4.80	4.35

Variable Definitions

Dependent Variable: Dummy variable = 1 if found work during nine months following reappraisal, 0 otherwise, based on follow-up questionnaires.

MASSWEP Satisfaction Index: (1) How much did you learn in the position in terms of skills, job knowledge, training or a trade (Likert 1-4), (2) Do you think the position will improve your chances for getting a regular job (Likert 1-4), (3) Do you think you are as pleased with the position as the regular employees are (Likert 1-3), (4) In general, how satisfied/dissatisfied are you with the position (Likert 1-4), (5) As a program to give men work experience, how would you rate the WEP Program? (1-10 scale), (6) Is the position something that interests you? (Yes/No). Alpha reliability .83.

Fate Control Index:
When I make plans, I am almost certain that I can make them work.
or
It is not always a good idea to plan too far ahead because many things in life turn out to be a matter of good or bad luck anyway.
and
Things usually come up to make me change my plans.
or
When I make plans, I usually get to carry things out the way I expected.
(Alpha Reliability .66)

Work Ethic Index:
A man really can't think well of himself unless he has a job.
and
Even if a person inherited enough money so that he/she could live comfortably without ever working, he/she should work anyway.
(Likert 1-4) Alpha Reliability .52.

Other variables as defined in Tables V-4 and V-7.

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E. Conclusion

The central finding of this chapter is that neither of the two treatments in MASSWEP, modification of the 100-hour rule and assignment to work experience sites, had statistically significant effects on the rate of job finding among men participating in this experiment. The raw data on job finding, when disaggregated by time and region, suggested some possibility that both of the treatments mattered. Thus, using WIN program data and aggregating job finding rates for all periods of the experiment, 34.2 percent of the men in Experimental Group II found unsubsidized jobs compared to 30.7 percent in Experimental Group I and 28.7 percent in the Control Group. The difference between the two experimental groups would isolate the impact of work assignments, while the difference between Experimental Group I and the Control Group would isolate the impact of the modification of the 100-hour rule. Again, a variety of statistical tests showed that once other variables were controlled for, these differences were not statistically significant. Whatever differences appear between the experimental groups arises among those in Experimental Group II who did and did not appear at work sites.

Interestingly, follow-up questionnaires distributed by Brandeis found job finding rates in all three experimental groups to be 17 to 20 percentage points higher than did data gathered through standard WIN program forms. (Thus, the job finding rates using the Brandeis follow-up data were 62.6 percent in Experimental Group II, 55.8 in Experimental Group I, and 57.0 percent in Control Group). This difference, not intended to be explained in this study, arises either through slow communication

between MDES/WIN and MDPW offices, or limited contacts between clients and MDES/WIN offices. or a high rate of concealing work from MDES/WIN among AFDC-UF clients.

Lastly, the high rate of job finding among men in all three groups indicates that many, although experiencing long-term unemployment are not unemployable. Almost three of five men in the Control Group found employment, with or without assistance from the standard WIN program, after six months of unemployment and within nine (additional) months following their participation in MASSWEP. The pattern common to other groups of recipients, of mixing work and welfare seems also to hold in this group. If work experience is a treatment appropriate for the hard core unemployed, it was given to many who did not fit this category. Of course, our sample contained a real hard core that could not find employment even after at least 15 months. However, work experience appears to have had very little effect, if any, on either those who did or did not find jobs.

CHAPTER VI

Welfare Payments and MASSWEP Costs

The effects of MASSWEP on work are of interest in themselves. In addition, the work choices of an individual will tend to affect the level of his welfare payments. A second major area of interest is how MASSWEP affects payment levels and thus overall welfare costs. This chapter investigates the effect of MASSWEP on AFDC-UF payments. This investigation then serves as a basis for a benefit-cost analysis of MASSWEP since a principal benefit of MASSWEP is intended to be a cut in welfare costs.

A. The Effect of MASSWEP on AFDC-UF Payments

This section will consider first the data available to us; second the methods for studying this data, and third our results.

1. Data on Welfare

In order to study welfare payments we obtained a tape from the welfare department giving monthly payments for our clients beginning with the last three months of 1977 (before the program began) and running through September 1979. Even so, our record is incomplete. We presented to the welfare department the social security numbers of all the men in our sample in order to select the cases to be included on the tape. Some numbers could not be matched. One reason apparently was that the welfare department listed the case in the wife's name. In addition the record available on individuals is sometimes not complete, in part because the welfare department had not fully computerized their operations in the early

days of the MASSWEP program. Nevertheless, we have at least some information on 815 clients during the program.

The monthly data pose a problem in that the basic grant and the flat supplementary grant are reported separately, but the flat grant is paid only once during the quarter. Adding these two together produces a very irregular series because the flat grant is not paid monthly. To smooth out these irregularities we combine the monthly payments into a quarterly figure the total of the basic grant and the flat grant over the whole quarter. Of course, in quarters when the person goes off or on welfare, the payment amount may not be typical, but in a large sample, these irregularities should not be substantial.

2. Methods of Study

The central question of interest is whether the MASSWEP treatments succeeded in reducing welfare payments. The difficulty in studying this is that AFDC-UF payments in general increased during a portion of the period of the program. In order to control for this fact, it is necessary to make use of our experimental design once more. Since payments in all treatment groups were rising, the critical question is how payments in the experimental groups compare with those in the control group as the program proceeded. Of course, there are differences among individuals within groups, too. Thus, to control for all relevant variables, treatment as well as other, regression will be used.

The question of payments can be broken into two parts: 1) Was the person on or off welfare? 2) If he was on, what was the level of benefits?

While we look at both parts separately, for the regression analysis we combinethem. To do so, we concentrate on the average quarterly benefit received by an individual. In studying the work impacts, we followed each individual over a nine month observation period following his reappraisal. To study payments on a comparable basis, we restrict attention to the calendar quarters during which the individual was in his nine month observation period. Then we take the average payment over these three quarters (or possibly four, if the person was reappraised in the middle of a calendar quarter). If the person is off welfare for an entire quarter, his payment is zero. His mean benefit will be reduced accordingly, but the mean will not be zero unless he was off over his entire nine month observation period, an unlikely occurrence given the nature of our sample. Variations in mean quarterly benefit will thus reflect not only variations in payment levels, but also in being on or off welfare.

It is necessary to choose control variables for the regression analysis. However, a preliminary examination of the payments data revealed a special problem; this will be considered before developing the regression model.

3. Results

We consider first some characteristics of the raw data and then present our regression analysis.

a. Payments Characteristics

Table VI-1 presents the quarterly record of payments for each treatment group, in terms of group means. Figure VI-1 graphs the patterns.

TABLE VI-I

AVERAGE PAYMENT PER QUARTER

Quarter	Control	Experimental I	Experimental II	Experimental II	
				Attended Sites	Did Not Attend Sites
1977 IV	679 (58)	838 (47)	818 (274)	910 (76)	783 (198)
1978 I	762 (50)	924 (43)	903 (219)	1024 (61)	856 (158)
1978 II	866 (58)	909 (46)	961 (269)	1090 (73)	914 (196)
1978 III	1005 (73)	816 (62)	951 (361)	1184 (105)	856 (256)
1978 IV	854 (80)	801 (69)	808 (394)	1101 (120)	679 (274)
1979 I	1041 (72)	1050 (61)	1066 (352)	1220 (133)	973 (219)
1979 II	684 (63)	810 (57)	871 (297)	954 (120)	814 (177)
1979 III	527 (50)	731 (46)	804 (319)	886 (93)	746 (130)
Average from 1978-I - 1979 III	839 (123)	864 (108)	925 (584)	1080 (198)	845 (386)

(Sample size in each cell given in parentheses.)

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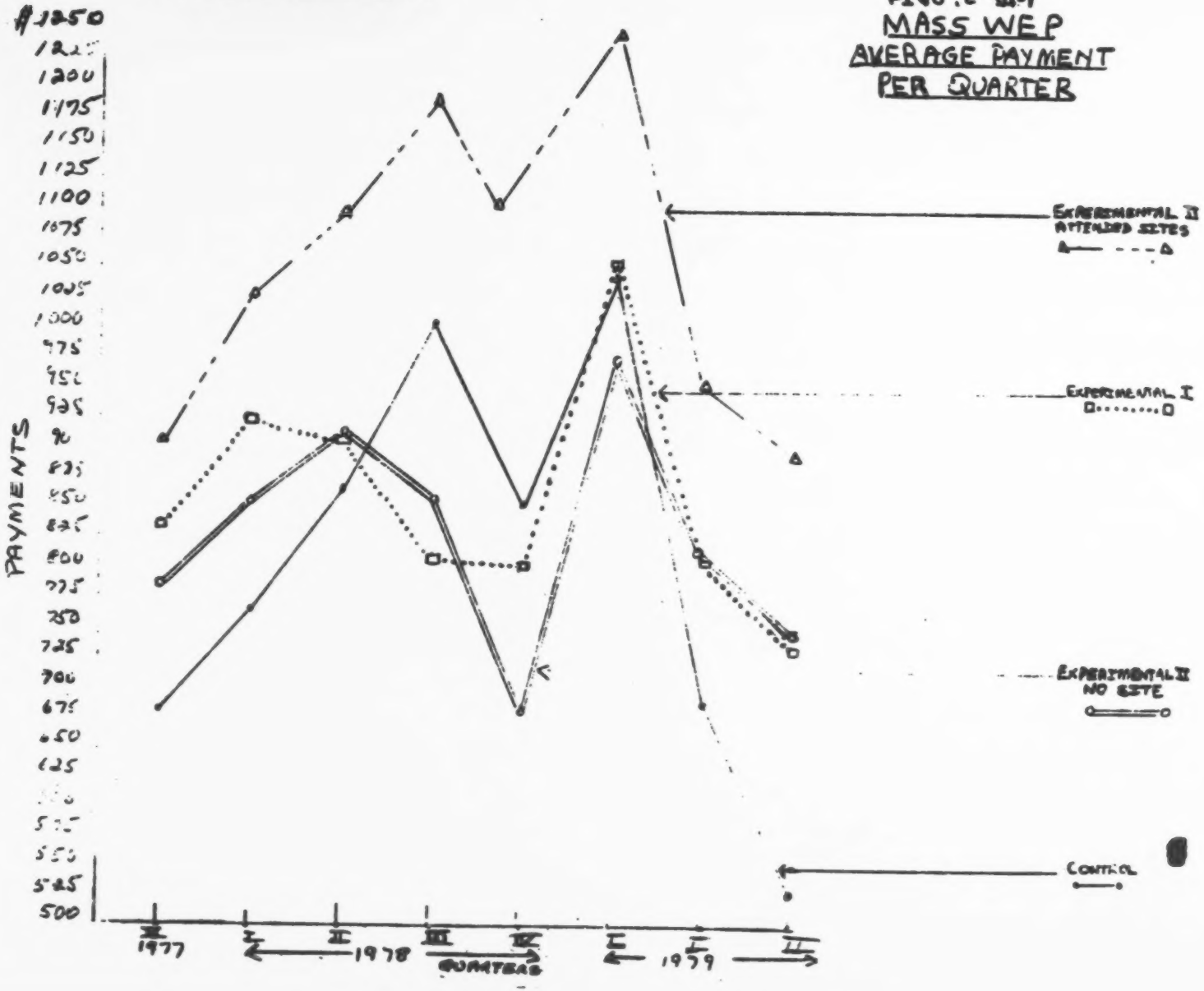
FIGURE III-1

MASS WEP
AVERAGE PAYMENT
PER QUARTER



The record begins with the pre-program quarter and then proceeds through seven quarters during which men were in their nine month observation period The cell sizes vary from quarter to quarter because only those in their nine month observation period are included in that quarter's sample.

There is a common pattern across all treatment groups in welfare payments over time. Payments rise more or less steadily through the third quarter of 1978, dip temporarily in the fourth, reach a new peak in the first quarter of 1979, and then come down. These parallel patterns confirm the difficulty in distinguishing the pure effect of MASSWEP and the need to make comparisons with the Control Group. Unfortunately, as with job finding data, sample sizes by quarter are small for reliable statistical testing which is why we rely on the mean over time for the regression analysis.

Table VI-1 does allow comparisons with the Control Group by quarter, at least in terms of averages. With the exception of the third and fourth quarters of 1978, quarterly payments to the Control Group measured in terms of the group mean, are almost always smaller than those to all other groups. Payments to those in Experimental Group II who attended WEP sites are in all quarters larger than average payments to all other groups. It would be tempting to conclude that the MASSWEP treatment raises--rather than lowers--welfare costs. Payments to Experimental Group I are generally higher than those to the Control Group indicating that the thirty-and-a third treatment is costly; by this line of argument the most costly treatment of all in terms of payment levels is to send a person to a WEP site. However, these conclusions are premature. One need only look at the pre-program quarter in 1977 to observe that the same pattern obtained

then before there had been any experimental effect. Apparently, there are differences between the groups in welfare experiences which predate MASSWEP and which therefore make direct comparisons between groups without further controls unreliable.

Table VI-1 concludes with the average quarterly benefit--averaged first for each individual over his nine month observation period and then across individuals within each group. The average is for all groups higher than the pre-program quarterly figure. The average, of course, cannot show the ups and downs by quarter and the fact that by the last quarter, group average payments were below pre-program levels. Nevertheless, as a single measure, it provides information on comparative payment levels across individuals or groups. It will serve as the dependent variable in the regression analysis.

Before proceeding with an analysis of individual average quarterly benefits, there is a further breakdown of the data to consider. Table VI-2 looks at the extent clients got off welfare altogether. If a person received a zero payment for at least one full quarter during his nine month observation period, then we classify him as a welfare leaver. Admittedly, this condition is too strong and may understate somewhat the extent of welfare leaving. However, since we restrict our study to effects over a nine month span for each client, being off welfare for less than a quarter would have only a negligible effect on program cost. In any case we should obtain a rough idea of the extent of welfare leaving. The table shows a substantially lower rate of welfare leaving among clients in Experimental Group II who attended sites. This is consistent with the earlier finding that this group has higher mean payments. This group also began with higher payments. Now we find that they left welfare at a slower rate. It appears

TABLE VI-2

Rate of Leaving Welfare

	Control	Experimental I	Experimental II	
			Attended WEP site	Did not attend WEP site
Percent leaving welfare				
in whole sample	42.3	35.2	25.8	45.3
among job finders	68.3	47.2	39.7	58.8
among those not finding jobs	12.5	13.6	2.0	18.6

that there is a special welfare problem concentrated within this group. The difference in welfare leaving probably does not reflect the effect of the MASSWEP treatment so much as this underlying problem which remains to be identified.

Table VI-2 also breaks down the rate of welfare leaving by whether the person ever found a job. For those in the Control Group, finding a full-time job (greater than 100 hours per month) will lead to ineligibility. For those in the other groups, since they are eligible for 30-and-1/3, finding a job will lead to ineligibility only if earnings exceed the (higher) breakeven level. Not surprisingly, the rate of welfare leaving is higher among job finders in the Control Group than in the groups eligible for 30-and-1/3. But even among job finders, the rate of leaving is noticeably lower among those in Experimental Group II who attended WEP sites. This would be possible if they tended to find lower paying jobs than others or if they had larger families so that they had higher breakeven levels, but we did not find strong evidence of either possibility. We found no evidence in Chapter V that these men had

a lower probability of finding a job. However, the result could be explained if they were slower in finding jobs so that fewer had as much as a full quarter off welfare. This is at least possible to the extent that men spent time at their WEP sites, but many did not. Of course, it remains to be established whether differences either in benefits or in welfare leaving between groups are statistically significant. For this we need to control for more variables by use of regression.

b. Regression Analysis

Welfare payments are determined by an exact formula so that if we had data on the variables entering the formula for an individual, regression would be inappropriate. The question of how MASSWEP affects payment ideally should be studied by examining how it affects the variables entering the payments formula rather than directly. Unfortunately, we do not have the same data available to the welfare department; even when we observe the same variables, they are generally observed at different times. Based on the data we have, we should not observe an exact formula relationship; there is a random element in the relationship we can observe. The regressions provide a test of experimental effect in addition to the direct effect on work effort.

The main variables affecting payments should be family size and earnings if eligible for 30-and-1/3, or the fact of full-time work if faced with the 100-hour requirement. We have a measure of family size. However, using either earnings or the fact of job finding would introduce the risk of simultaneity bias since both our payments variable (mean over time) and

a work or earnings variable would be determined over the same time period in interaction with each other. To avoid this problem we replace the work or earnings variable with a set of exogenous variables which might be expected to explain work or earnings. These are variables which on their own should not be related directly to payments but which serve as proxies for a work measure. MASSWEP treatments should matter only indirectly, as determinants of work behavior. To detect both this indirect effect as well as the possibility that they might somehow matter directly, we include them explicitly. Finally, in view of our finding that large differences in payments between treatment groups existed even before the start of the program, we include as a control variable the level of payments for each individual in the quarter before his nine month observation level. This variable should serve as a control for what appear to be substantial variations between individuals that we otherwise cannot measure directly.

Results are reported in Table VI-3. Not surprisingly, family size is strongly significant in the expected direction. Similarly, the pre-program payment level is highly significant, indicating that we are not measuring directly important explanatory factors. Non-whites have significantly higher benefits, corresponding to the finding in Table V-7 that they have a significantly lower probability of finding a job (at least when data from the follow-up questionnaires is used). The other significant variables in explaining job finding--the health variables--were not included here because they are available only on the follow-up questionnaires; in order to make fullest use of the welfare data, we wanted to include all

TABLE VI-3

Mean Welfare Payments

Family Size	38.66*
	(10.75)
Age Under 25	-54.69
	(49.77)
Age Over 40	-62.87
	(36.18)
High School Graduate	-30.55
	(36.49)
Attended College	-45.34
	(56.48)
Non-white	103.4*
	50.03
Months of Unemployment	.7289
	.8159
Months on Last Job	.2279
	(.3041)
Wage on Previous Job	-14.75
	(8.094)
Pre-Program Payment	.5692*
	.0302
Attended WEP Site	216.2*
	(39.59)
Experimental I	41.75
	(57.60)
Experimental II	16.15
	45.87
Constant	74.99
R^2	.5906
n	449
F	48.27

Dependent variable: mean quarterly payments over the period of the client's nine-month observation period.

All other variables as defined in Table V-7.

cases for which welfare information was available even if a follow-up questionnaire had not been completed. The only other significant variable is whether the person attended a WEP site (which necessarily places him in Experimental Group II). The sign is positive, indicating a higher payment level for those who go through the full MASSWEP treatment. We have already observed that the mean of welfare payments was higher for those attending WEP sites than for all others, but that this relationship had prevailed even before the start of MASSWEP. In the regression analysis we control for previous benefit level which is, of course, significant, but even beyond this control, there remains a positive relationship between benefit level and attending a WEP site. It seems unlikely that this could be a casual relationship. If MASSWEP were to succeed, it would stimulate work effort and reduce payment levels. We earlier found no effect on the rate of job finding and now no effect in reducing benefits. It is possible that attending WEP sites could actually delay a return to work and thus result in the higher payment levels observed. However, the number of clients spending as much as 13 weeks at a WEP site was small enough that this explanation is unlikely to account fully for the higher payment levels to those attending WEP sites. The likeliest explanation perhaps is that those sent to WEP sites had more difficult welfare problems than others. The effect of these problems is more complex than can be detected by the previous payment level or any of the other measurable control variables. Indeed, the difficulty with this hypothesis is that we cannot uncover what the source of the problems may be. Whatever the explanation, it appears that these people had higher benefits and that MASSWEP is not the sole cause--and perhaps not responsible at all.

B. Cost Benefit Analysis

We shall first present the costs of MASSWEP and then discuss measures of benefits and costs associated with the program.

1. Costs of MASSWEP

Table VI-4

Costs of MASSWEP

	FY 78	FY 79	Total
Personnel (staff) services (including benefits)	$72,548	165,899	238,447
Non-personnel expenses	1,578	1,420	2,998
Work Experience Allowances (paid to clients attending sites)	9,381	18,766	28,147
	$83,507	$186,085	$269,592

In studying costs we rely on the figures of MDES. For personnel services the cost estimates depend on the time reports of MDES staff who recorded the daily amount of time devoted to MASSWEP. In estimating non personnel expenses, care seems to have been taken in identifying costs attributed solely to MASSWEP, although we cannot verify independently the reported amounts. The program began in January 1978 and ran until September 1979. Table VI-4 reports MASSWEP costs during fiscal years 1978 and 1979 separately and combined.

These figures measure the costs explicitly charged to MASSWEP and paid either by the federal WIN program ($244,696 on the whole program) or the approximately 10 percent state match ($24,895). These figures

do not represent the full cost of operating MASSWEP, however, since a number of CETA workers were used during part of the program. Their cost was not charged to either the federal WIN program or the state, even though they played a significant part in the program. The state estimates their cost at $190,000 (40 personnel at $9500 per year, but working an average of six months). Adding the CETA costs, the total program cost becomes $459,592.

Since MASSWEP had a number of parts, it would be useful to allocate costs to the paper selection, reappraisal, etc. Unfortunately, no data are available on such a breakdown. The MDES accounting sheets did try to break down costs into the standard WIN treatments. It is true that some men going through paper selection or reappraisal may have been sent on to OJT or PSE as reported on the accounting sheets. However, while costs on these treatments are reported, the breakdown does not separate the costs for either the paper selection or reappraisal. It appears that the assignment of costs to the standard WIN treatment categories was essentially arbitrary at the state level. We concluded that reliable information was not available on MASSWEP components separately.

As a sideline to the cost data, it might be noted that the original contract with Massachusetts from the federal WIN program budgeted approximately $70,000 for social services to be arranged by SAU workers. However, none of this allocation was used.

2. Relating Benefits and Costs

Both the screening process in MASSWEP and the work experience treatment (together with the modification in the 100 hour limitation)

might have yielded benefits. The screening process--the combination of the paper selection and reappraisal--may have succeeded in eliminating ineligible clients from the unassigned recipient pool, thus producing welfare savings. It may also have sent clients to treatments they needed, but were not obtaining, another potential source of benefit. Unfortunately, we do not have information on the numbers of clients eliminated from the rolls or sent to other treatments. We did find that large numbers in the unassigned recipient pool were not found appropriate for work experience. Yet this cannot be taken as a measure of success for the screening process since many of these were finding jobs through normal processes not related to MASSWEP. While we presented qualitative information on the screening processes in Chapter IV, neither our experimental design nor our independent gathering of quantitative data were designed to measure many of the separate benefits that could result from the screening. While it appears likely that such benefits did exist, it would be purest speculation to assign values to the benefits. Furthermore, we lack data on the cost of the screening processes separately.

The experimental design and data gathering procedures were intended to provide information on the work experience treatment and the modification in the 100 hour limitation. Two types of benefits could arise from these treatments: work in regular jobs might increase and welfare payments might go down. The two types of benefits are, of course, related since higher earnings from work will generally reduce welfare

payments. In view of this dependence, we would overstate total benefits if the benefits from higher earnings were added to the benefits of lower welfare payments. Let ΔE represent the increment in the earnings of a client attributable to a MASSWEP treatment and ΔW the corresponding increment in welfare payments. ΔW is a clear benefit to the state, provided it is a saving. But ΔW is determined by ΔE so that we must consider whether it is necessary to estimate benefits from ΔE separately. Considering the problem from the point of view of the client, his income, I, depends on both his earnings and his welfare payments. In particular the increment in his income attributable to MASSWEP is the increment in his earnings less what he loses in welfare payments as a result:

$$\Delta I = \Delta E - \Delta W$$

This expression allows us to decompose ΔE into two parts:

$$\Delta E = \Delta W + \Delta I$$

A part of ΔE simply replaces ΔW, welfare payments lost, and is thus already accounted for in the measurement of ΔW directly. This leaves ΔI, the net gain to the individual resulting from the MASSWEP program, unaccounted for. (We ignore here effects that might extend over time beyond the duration of the experiment.) It appears then that what ought to be measured beyond ΔW is ΔI. On the other hand, it could be argued that from the point of view of the state, ΔI does not need to be included as a benefit since it is strictly a private benefit: the individual makes more, but he captures this amount solely for himself. Under this argument the only benefit that needs to be measured is a ΔW.

For this program the analysis is simplified because of our empirical results. In Chapter V we found no significant effect of MASSWEP on the probability of finding a job. Sample sizes were too small to obtain

tests of significance of effects on wages, hours, or earnings, but the differences between treatment groups did not appear large. Our findings are consistent with the conclusion that the work experience component had no significant impact on earnings; if there was an effect it was small and could not be measured reliably. Confirming this conclusion was our finding in this chapter that MASSWEP had no significant impact on welfare payments. While welfare payments declined during the experiment, they declined among all treatment groups; none of the decline could be attributed to MASSWEP. In short our results found no evidence of any benefits from the work experience component of MASSWEP. If benefits did exist, they were apparently too small to be measured with any reliability.

Costs need to be measured on a basis consistent with benefits. Benefits were measured per person--the change in average earnings or welfare payments per person. Similarly, costs need to be measured per person. The problem is to decide which population to use. Since we are concentrating on the work experience component of MASSWEP, we should look at those who were to receive this treatment, but a qualification is necessary. Our cost figures include the cost of the screening as well as arranging for work experience. If we calculate cost per person appropriate for work experience, we are combining two costs associated with this treatment. First, there is the cost of identifying the appropriates: we allocate the whole cost of the screening to those found appropriate. Ideally, it would have been preferable to measure benefits and costs associated with the inappropriates, but being unable to do this, we are in effect charging the entire cost of the screening against the work experience treatment. Added to the cost of the screening is the cost of administering the work experience component.

The cost of the program was $269,592, or, including CETA costs, $459,592. There were 1033 appropriates. This means a cost of $261 per client or $445 including CETA costs.[1] In view of our finding of no significant benefits, work experience seems to fail the cost benefit test. Even if we allow that costs are inflated because they include costs of the screening which may have produced its own benefits, the work experience component remains cost ineffective because we could identify no benefits from it.

C. Conclusion

Neither of the two treatments tried in MASSWEP lead to a reduction in welfare payments among the men in the experimental groups. In light of the fact that MASSWEP did not increase the rate of job finding among participants, the lack of effect regarding welfare payments is not entirely surprising. A direct negative effect on payments of assignment to sites was possible. People might have felt that the cost of participating in AFDC-UF had become too high and were thus impelled to leave welfare even without jobs. However, this was not the case. We were unable to uncover any impact of the two treatments either via the route of employment or directly through welfare leaving.

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[1] Since this was an experiment, not all the costs reflect the costs of an ongoing program. We do not know what fraction of the costs went to administer the experiment. It may be assumed that even the screening costs of those who ended up in the control group should not be charged as an expense of work experience. In all 28 percent of the appropriates received treatments other than work experience. As a rough guess we could thus count only 72 percent of costs as attributable to work experience. But at the same time only 72 percent of the population of appropriates should be included in the denominator. Thus cost per appropriate in the work experience treatment group remains the same as the .72 cancels from both numerator and denominator.

The program cost $445 per appropriate client (including the cost of CETA workers used for a time by MDES). However, in the absence of detectable benefits, the work experience treatment as administered in MASSWEP appears to have been cost ineffective.

CHAPTER VII

Policy Considerations

In view of our research findings we may now address two central policy questions: 1) Is mandatory work experience--or similarly workfare--an appropriate treatment for its targeted population or are there more suitable treatments for these clients? 2) Given that mandatory work experience did not function well in MASSWEP, could it be made to operate more satisfactorily in other places?

Consider first the suitability of mandatory work experience for its target population. There are two approaches to mandatory work programs. The workfare approach would assign virtually all registrants to work sites. In contrast, mandatory work experience could be viewed as an assistive service: men long unemployed might need work experience to establish the credentials and work habits necessary to be considered for regular jobs. Considered as an assistive service, work experience may be appropriate for some clients while others would benefit more from other services. Although there was some confusion about the objectives of MASSWEP, it more or less followed the assistive approach, with the screening used to assign clients to other WIN treatments if these would be more beneficial. Our results allow us to draw some conclusions concerning the size of the target population suitable for work experience as an assistive service.

- In the initial planning stage for MASSWEP the size of the likely target population was greatly exaggerated. It was originally expected that the screening processes would find about 2800 men suitable for work experience, but only 1033 were actually found once all the inappropriates were screened out. Given the irregularities in the screening process, the 1033 figure may be too low, but it is likely that the true figure would also be less than early expectations.

Overestimation of the likely numbers has occurred in other workfare programs as well. Apparently, advocates of workfare tend to exaggerate the potential need for such a program. The underestimation by MDES of the extent of work among registrants probably helped contribute to this exaggerated impression of the need for work experience. Indeed any program with irregular and infrequent contact with clients--as certainly was the case in the unassigned recipient pool--will be able to provide only very imperfect information on the work effort of its clients.

- Although the screening processes found 1033 clients more suitable for work experience than for any existing WIN treatment, this appears to have been an erroneous judgment in many cases, at least if work experience is viewed as an assistive service. The screening process, like the planning stage, overestimated the number of men who could benefit from work experience.

Fifty-seven percent of men in the Control Group--all long-term unemployed--found jobs without any work experience treatment. We may conclude that the long unemployment spells of these men did not constitute an insurmountable barrier to employment. For such men, work experience is likely to provide fewer benefits than other assistive services. If there were a need for work experience, and even this is dubious in view of our results, it must have been concentrated among

the remaining 43 percent, about 445 men. This reinforces the idea that the need for work experience was greatly overestimated in planning for MASSWEP.

- The number of men who would benefit from a well-functioning work experience program is limited. Presumably, it is just some subset of the 45 percent who were not finding jobs. Voluntary work experience for women has been shown to yield benefits. However, most of the men in our target group have a fair amount of labor market experience in contrast to the women who voluntarily participate in work experience.

Considering the job finders in the Control Group, they were very slow in finding work, although they were not unemployable. What they needed would be a treatment to speed their return to work. Besides its assistive aspect, work experience could have intimidated some into finding work sooner, although this effect seems to have been minor in MASSWEP. However, would any alternative work better?

- For the men who were finding jobs anyway, but very slowly, we may speculate that the regular WIN treatments would have been as effective as any other. The trouble with these treatments is that men in the target population were not receiving them. These men had work problems and probably could have benefited from the combination of pressure and assistance offered by WIN. But to be effective, MDES would need regular and frequent contact with these men, something that had been lacking.

- Although mandatory work experience is likely to have only limited effect in assisting men to overcome labor market problems, it may have appeal as a punitive measure. A strictly enforced program will either intimidate registrants into

leaving welfare altogether or will force them to work without pay at special jobs. From the point of view of welfare costs, there will be savings to the extent that men are intimidated off of welfare, but not to the extent that they go to work experience sites without benefiting.

Consider now the possibilities that a mandatory work experience program might operate more successfully than MASSWEP. We know that voluntary work experience for women has yielded benefits; also, some workfare programs have functioned over prolonged periods, although there have not been rigorous evaluations of their effects on work and on welfare costs. Some of the difficulties with MASSWEP were implementation problems associated with its introduction. But some could lead to ongoing complications.

- Mandatory work experience and workfare are sensitive political subjects with strong feelings on both sides. There was extensive lobbying in the planning stage of MASSWEP. There were legal pressures and even an injunction. The courts are now more sensitive to issues of due process in mandatory programs. Differences in political views among WIN staff contributed to some of the irregularity in what happened across local offices. These problems of political conflict are likely to appear in any new workfare program.

Although voluntary work experience is inexpensive to operate and may appear to offer a cheap alternative to other WIN treatments, the mandatory element will make such a program more expensive and more complicated to run. The following four items spell out some sources of higher cost.

- A mandatory activity requires frequent contact with clients together with monitoring of their activities. This can be both expensive and complicated, to the extent clients try to evade the intent of the program.

- For any work experience program, work sites must be chosen carefully. In MASSWEP some clients left work sites quickly because they did not want to work, but in many cases the work sites were just not suitable. Developing sites and assigning clients to suitable ones can be costly.

- Men sent to work sites on a mandatory basis can pose problems for the work sponsor in terms of discipline and productivity. If there are implicit (although not direct monetary) costs to work sponsors, they may be reluctant to participate, adding to the costs of the MDES or similar agency for job development.

- In a mandatory program, adjudication procedures must come into use whenever a client fails to comply with program requirements. To insure due process, these procedures are slow and complicated. If sizable numbers of clients do not comply with rules, adjudication costs can become substantial.

In MASSWEP there was a failure to develop sites carefully and to deal with clients who left work experience sites early. Had these problems been dealt with, the others might have also appeared. Nevertheless, it is conceivable that with sufficient resources and administrative control a mandatory work experience program could be made to function. Whether it would be more successful than MASSWEP in increasing employment in regular jobs or in reducing welfare costs is problematical, but our previous arguments suggest that the number of potential gainers is small. At the same time, the cost of an effective program is likely to be higher than that of MASSWEP.

The basic problem is that getting people to work in regular jobs and thereby reducing welfare costs is expensive. Whether considering assistive services or coercive pressures, it is not clear that we are prepared to spend--or even ought to--all the resources necessary to achieve success. Mandatory work experience in particular is likely to benefit such a small population that even a smoothly functioning program probably would not be cost effective. The particular program tried in MASSWEP certain was not.

APPENDIX A

Data Sources for Evaluation of MASSWEP

Data Collection Form	Information Secured	Obtained From
Standard State Forms:		
1. ESARS/Work History (ES-511-W)	Basic client demographic, income and employment information collected at time client registers with WIN	Local WIN offices
2. WIN Status Change Notice (MA 5-97)	Changes in client WIN status and related changes in employment and welfare status	Local WIN offices
3. Payment Order Time Card (ETA 2-148-A)	Number of hours and pay periods and amount paid to each client who went to a work site	Retraining Claims Unit, Boston
4. MDPW computer printouts	Monthly welfare payments to AFDC-UF families	MDPW
Forms Developed by Brandeis:		
1. WIN Worker Form and SAU Worker Form	Information regarding client's educational and vocational barriers to employment and emotional and medical barriers to employment filled out at time of reappraisal [a]	Mailed to Brandeis by WIN interviewer; Mailed to Brandeis by SAU
2. Personal Interview Guide for WEP Clients[b] (N = 174)	Type of work site, working conditions and client's perceptions of the work site experience	Personal interviews with men who had been or were on work sites

cont'd.

APPENDIX A

Cont'd.

Data Collection Form	Information Secured	Obtained From
3. Mailed survey[b] questionanaire designed specifically for each of the three groups[c] (N = 542)	Work patterns, income and perceptions of WEP obtained approximately nine months after each client's reappraisal	Self administered by client and returned by mail to Brandeis; also interviewer administered
4. WEP Work Sponsor Interview (N = 60)	Type of sponsor, sponsor's attitudes toward WEP and perception of the client	Personal interview
5. Personal Interview Instrument with: SAU/ Welfare Counselor; and with DES/WIN counselor (N = 41)	Perceptions about the process of implementing MASSWEP	Personal interview

a/ The total number found appropriate in the reappraisal was 1015. Of these WIN barrier forms were received for 885 and SAU barrier forms for 587. A total of 370 were found inappropriate in the reappraisal. Of these, WIN barrier forms were received for 333 and SAU barrier forms for 64.

b/ Men who completed the personal interview were mailed a $7.00 check by the Brandeis researchers and those completing the mailed survey received a $5.00 check.

c/ Note that those in Experimental II received a questionnaire based on whether they went or did not go to a work site.

APPENDIX B

SAU WORKER FORM

Name of SAU Worker_____

SAU Community Service Area Office_____

NAME OF CLIENT_____

Client's Social Security Number_____

Date of Interview_____

Listed below are common types of problems which can interfere with getting or keeping a job. Please read through the entire list before conducting assessment interviews. Then fill out this form during the interview. Check the YES box for an item if the client appears to have that type of problem <u>with respect to getting or keeping a job</u>. Otherwise, check the NO box.

1.	Needs child care	_/yes _/no
2.	Medical problems or physical handicaps (include any type of medical problem including alcoholism or drug use)	_/yes _/no
	a. If yes, does this problem restrict whether or not client can work?	_/yes _/no
	b. Does this problem restrict the kind of work the client can do?	_/yes _/no
3.	Transportation problems	_/yes _/no
4.	Legal entanglements	_/yes _/no
5.	Client indicates that he has marital difficulties	_/yes _/no
6.	Client indicates that he has difficulty getting along with others	_/yes _/no
7.	Client indicates that he is tense and nervous	_/yes _/no
8.	Client says that he suffers from depression	_/yes _/no
9.	Did any of the problems, 1-8, cause the client to lose his last job?	_/yes _/no

If yes, list by number_____

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APPENDIX B

WIN WORKER FORM

Cost Center Number _____

Name of WIN Worker _____

NAME OF CLIENT _____

Client's Social Security Number _____

Date of Interview _____

How long has it been since the client had a full-time job? _____ months.

How long did client work at last full-time job? _____ months.

Listed below are common types of problems which can interfere with getting or keeping a job. Please read through the entire list before conducting assessment interviews. Then fill out this form during the interview. Check the YES box for an item if the client appears to have that type of problem with respect to getting or keeping a job. Otherwise, check the NO box.

1.	Lacks education	_/yes	_/no
2.	Has little previous job experience	_/yes	_/no
3.	Lacks specialized skills	_/yes	_/no
4.	Lack of references	_/yes	_/no
5.	Has difficulties in English	_/yes	_/no
6.	Has difficulties reading or writing	_/yes	_/no
7.	Looked for work but could not obtain job offer	_/yes	_.no
8.	Wanted to look for work, but lacked confidence	_/yes	_/no
9.	Did any of the problems, 1-8, cause the client to lose his last job?	_/yes	_/no

a. If yes, list by number _____

(see other side)

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WIN WORKER FORM - 2

FINAL ASSESSMENT (check one box)

_/ suitable for SI-WEP
_/ vocational rehabilitation
_/ obtain medical report
_/ no further action
_/ other (Specify, e.g., WIN-OJT or other WIN component)

_____

If this client is suitable for SI-WEP call (617) 647-2934 at the Heller School, Brandeis University. Obtain his random assignment and indicate what it is below:

_/ control group 1
_/ control group 2 (exempt from 100-hour requirement)
_/ SI-WEP experimental group

If the individual was assigned to the SI-WEP experimental group, answer the following at the conclusion of the second interview.

1. Were arrangements made during the interview to assign the client to a SI-WEP job? _/yes _/no

2. If yes, name the employer_____

   Location of employment_____

3. Did the client object to being assigned to SI-WEP job?

   ___strongly ____mildly ___no objection raised

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APPENDIX C

Pre-Treatment Client Characteristics

Introduction

This appendix examines client demographic and work history characteristics as observed before the client began his time in a treatment group. In effect it examines the base-line data of the program. The clients, of course, all share the experience of at least six months of employment. This appendix examines whether any other pre-treatment characteristics are shared.

A. Client Demographic Characteristics

Our data on pre-treatment demographic and work history characteristics of clients come from the Work History and ESARS forms filled out by MDES/WIN workers when the client entered WIN and then updated from time to time. Generally, it is information that was recorded well before the time of reappraisal for MASSWEP. In addition some pre-program information comes from the WIN and SAU Barrier Forms which were supposed to be filled out at the time of reappraisal.

Those found appropriate in the reappraisal were selected supposedly from among the most difficult clients in the WIN caseload: they were the relatively long term unemployed who were in the WIN unassigned recipient pool for some time who also were not suitable for any other WIN treatment. It is thus interesting to consider whether any demographic characteristics differentiate MASSWEP clients from the general WIN population. The state of Massachusetts conducted studies of its WIN population which allow the comparisons shown in Table Appendix B-1.

The AFDC-UF fathers who qualified for the MASSWEP experiment are a somewhat more disadvantaged group than either a cohort of new AFDC-UF fathers or than a cohort of former AFDC-UF fathers, both from Massachusetts during the period of the experiment. As indicated in Table Appendix B-1 the MASSWEP participants have a higher average age than either the new or former recipients, and thus undoubtedly have more older workers among them. The average level of educational attainment is slightly lower in the MASSWEP group than in the two other samples. While the proportion of fathers who come from racial or ethnic minorities is the same in all three groups, the MASSWEP participants have slightly larger families. A stronger indicator of disadvantage in the MASSWEP group is the fact that only 57 percent of them earned in excess of $3.50 per hour on the jobs they held prior to coming on AFDC-UF. This contrasts with 74 percent of the fathers in the group of new AFDC-UF fathers and 73 percent of the men in the group of former AFDC-UF fathers.

Sketchy data suggest that the men in MASSWEP constitute a more dependent group than the former AFDC-UF fathers. The average length of the most recent spell on AFDC-UF for those in the group of former recipients was in the neighborhood of 11 months. While data were not available on the length of the current (unended) spell for those in the MASSWEP group, information on the length of the most recent spell of unemployment showed that it averaged over 25 months. Even if the men had spent an average of six months on Unemployment Insurance prior to entering AFDC-UF, it is likely that their still-unended spells average substantially more than 11 months.

The somewhat greater degree of disadvantage observed among the MASSWEP group should not be surprising. A cohort of new cases does not include an

Table Appendix C-1

Demographic Characteristics of Men
in MASSWEP and Men in MDPW/AFDC-UF Caseload

	MASSWEP AFDC-UF Fathers[a]	New AFDC-UF Fathers August 1977 Sample[b]	New AFDC-UF Fathers September 1977 Sample[b]	Former AFDC-UF Fathers August 1977 Sample[b]	Former AFDC-UF Fathers September 1977 Sample[b]
Average Age	37.0	31.6	31.4	31.7	32.0
Average Grades Completed	10.0	10.6	10.5	10.2	10.1
Percent White (Non-Hispanic)	90	85	89	90	86
Average Number of Children	2.8	2.2	2.2	2.3	2.3
Present or Most Recent Hourly Wage:					
Less than $2.50	14.6%	6%	8%	4%	3%
$2.51-$3.00	13.4	9	9	10	10
$3.01-$3.50	14.8	11	13	13	11
$3.51-$4.00	16.3	11	6	25	23
Over $4.00	40.8	63	64	48	53

a/ Source: Demographic and wage information from ESARS forms collected at local WIN offices.

b/ Source: L. Bittman, D. Balboni, M. Abramowitz, "A Survey of AFDC Case Openings and Closings in August and September 1977," MDPW, March 1978, Tables II and IV.

accumulation of long-term welfare cases. A cohort of closed cases should contain disproportionately, by the fact of their being closed, the "best" cases. The MASSWEP group was selected in a way that assured that it contained an accumulation of many of the most difficult AFDC-UF cases.

One must be careful, however, not to exaggerate the differences between the MASSWEP and the regular caseload. The fact that men in MASSWEP have, on average, slightly larger families may signal the fact that such men cannot afford to leave welfare, given the comparison of total welfare benefits (inclusive of Medicaid) and total family earnings. Men heading larger families may have capacities to earn amounts equal to those heading smaller families, but simply may find it more disadvantageous to return to full-time, reported work. Thus, the differences in labor market potential between the MASSWEP and regular caseloads could be in the indicated direction--but small.

Table Appendix B-2 presents information on the distributions of several demographic variables for the MASSWEP population. In addition the distributions are presented separately for each of the three treatment groups of the experiment. Although clients were assigned to the groups randomly, this comparison allows an inspection of whether there are systematic differences between the groups. The table shows no obvious pattern in the differences among the three experimental groups.

B. Client Work Histories

To complete the picture of clients entering MASSWEP, Tables Appendix B-3 and B-4 present information on work histories, with a breakdown again by

TABLE APPENDIX C-2

Demographic Characteristics of Men in MASSWEP Control and Experimental Groups

	Control Group	Experimental I	Experimental II
Age[a] (Percents)	(N=146)	(N=137)	(N=705)
14-24	11.6	11.7	9.8
25-39	47.3	54.7	52.3
40-54	37.7	31.4	33.0
55-69	3.4	2.2	4.8
	100.0	100.0	99.9
Education[a] (Percents)	(N=145)	(N=138)	(N=699)
0-11	61.4	64.5	62.5
12	30.3	25.4	26.0
13+	8.3	10.1	11.4
	100.0	100.0	99.9
Race[a] (Percent White and Non-Hispanic)	89.6	92.8	89.8
Average No. of Children in Family[a]	4.7	4.8	4.8
Father's Self-Reported Health Score:[b] 1 = good 2 = average 3 = poor	1.6	1.4	1.6

a/ Source: ESAR forms collected at local WIN offices.

b/ Source: Follow-up questionnaires.

TABLE APPENDIX C-3

Employment Characteristics of MASSHEP Participants, by Experimental Group

A. Previous Occupations[a]

	Control Group (%) (N=137)	Experimental I (%) (N=127)	Experimental II (%) (N=667)
Professional, Technical, Managerial	9.5	7.1	7.7
Clerical and Sales	10.9	9.4	9.7
Service	10.2	12.6	13.2
Agricultural, Fishery	5.1	1.6	3.0
Processing	2.9	3.1	4.1
Machine Trades	12.4	12.6	9.0
Benchwork	6.6	7.1	5.2
Structural Work	19.7	17.3	23.6
Miscellaneous	22.6	29.1	24.5
	99.9	99.9	100.0

B. Previous Wage Rates[a]

	(N=117)	(N=112)	(N=574)
Less than $2.50	16.2	14.3	14.3
$2.51-$3.00	9.4	14.3	14.1
$3.01-$3.50	15.4	12.5	15.2
$3.51-$4.00	18.8	21.4	14.8
$4.01 or more	40.2	37.5	41.6
	100.0	100.0	100.0

a/ Source: Work history section of ESARS forms collected at local WIN offices.

TABLE APPENDIX C-4

Work History Characteristics of MASSWEP Participants, By Experimental Group

	Control Group (%)	Experimental I (%)	Experimental II Attended Sites (%)	Experimental II Did Not Attend Sites (%)
Reasons for Leaving Last Job[a]				
Quit	8.2	12.1	9.4	14.8
Fired	18.0	14.7	11.1	12.4
Laid Off	23.8	32.8	33.2	26.7
Medical	10.7	9.5	9.1	11.0
Job Ended	18.9	15.5	17.5	16.7
Other	20.5	15.5	19.7	18.6
	100.1	100.1	100.0	100.0
Average Months of Last Job[a]	46	32	35	35
Average Months of Unemployment[a]	28	23	29	25
Percents which were:[b]				
Stable	44.9	29.5	39.7	38.4
Intermediate	33.9	41.1	36.4	36.6
Unstable	21.7	29.5	23.9	24.6

a/ Source: WIN worker Barrier Forms.

b/ Source: Work History Section of ESARS forms collected at local WIN offices.

experimental treatment group. Data in these tables were taken primarily from the client's Work History forms of MDES/WIN. The forms provide several pieces of information on each past job, although the completeness and accuracy of the information can be questioned. We made use of information on the client's three most recent jobs, to the extent that it was available. It should be remembered that everyone in this population has worked at some time since this is a condition for being included in the MASSWEP experiment.

The Dictionary of Occupational Titles code is used by WIN personnel to document occupations of new registrants. In some instances the DOT code represented a more skilled level of work than the work that the client had done recently, as indicated by the Work History, however, the DOT code in the ESAR was used. The data on the occupations of MASSWEP participants in their last jobs prior to entering AFDC-UF for their current welfare spells do not reveal any notable differences among the four groups.

The data on wage rates at the most recent job indicate that persons in the Control Group have been paid at rates somewhat above those received by men in the other two groups. While 74.4 percent of the men in the Control Group were paid over three dollars per hour on their last jobs, the percent above three dollars is virtually identical in the other two groups, and is three percentage points lower than in the Control Group.

Information from the WIN Barrier Forms indicated that the men in the Control Group held their most recent full-time job substantially longer than did the men in the other three groups.[1] The mean length of the last full-time

[1] This table has an additional group. Data for Experimental II was analyzed for those who attended sites and for those who did not attend sites.

job was 46 months in the Control Group and 11 to 14 months less in the other groups. But there were no observable differences in the reasons for which those jobs ended. There is no indication, for example, that voluntary separations were more pronounced than were involuntary separations in one group as opposed to the others.

On yet another indicator of previous work history, the men in the Control Group seemed to fare better than did those in the other three groups. In Table Appendix B-4, it can be seen that an examination of the last three jobs of each of the participants in MASSWEP showed that 45 percent of those in the Control Group had relatively stable work histories whereas in the other three groups this proportion varied between 30 and 40 percent. Jobs were considered short or long depending upon whether they lasted for 15 months, the median length of the last three jobs held by the men in the entire sample. Individuals were rated as stable or otherwise depending upon their score on the combination of their last three jobs.[2] Thus, on this measure of stability

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[2] By combining the designation for the three jobs as shown below ("blank" was used when there was no record of a different job) each client's work history was assigned to the Stable or Unstable or Intermediate Group.

GROUP	1st Job	2nd Job	3rd Job
Stable	long	long	long
	long	short	long
	long	long	blank
	long	blank	blank
Unstable	short	short	short
	short	short	blank
	short	blank	blank
Intermediate	all others		

as well as on that regarding the length of their last job, the men in the Control Group seemed to have better work histories than did those in the other two groups. Between the latter, men in Experimental I fared poorly as compared to men in the other group.

APPENDIX D

Numerical Client Flows Between Stages of MASSWEP

Chapter IV traced the steps of the MASSWEP program and analyzed what happened at each. At each step, some clients went on to the next, while others did not. Chapter IV tried to explain why clients left MASSWEP or otherwise did not move on at each step, but did not account for the flows numerically. Unfortunately, our data are not adequate to provede a complete accounting. However, this section will discuss the data we have together with some plausible inferences on the numerical sizes of the various flows. Figure D-1 summarizes the flows.

A. Client Flows Prior to Paper Selection

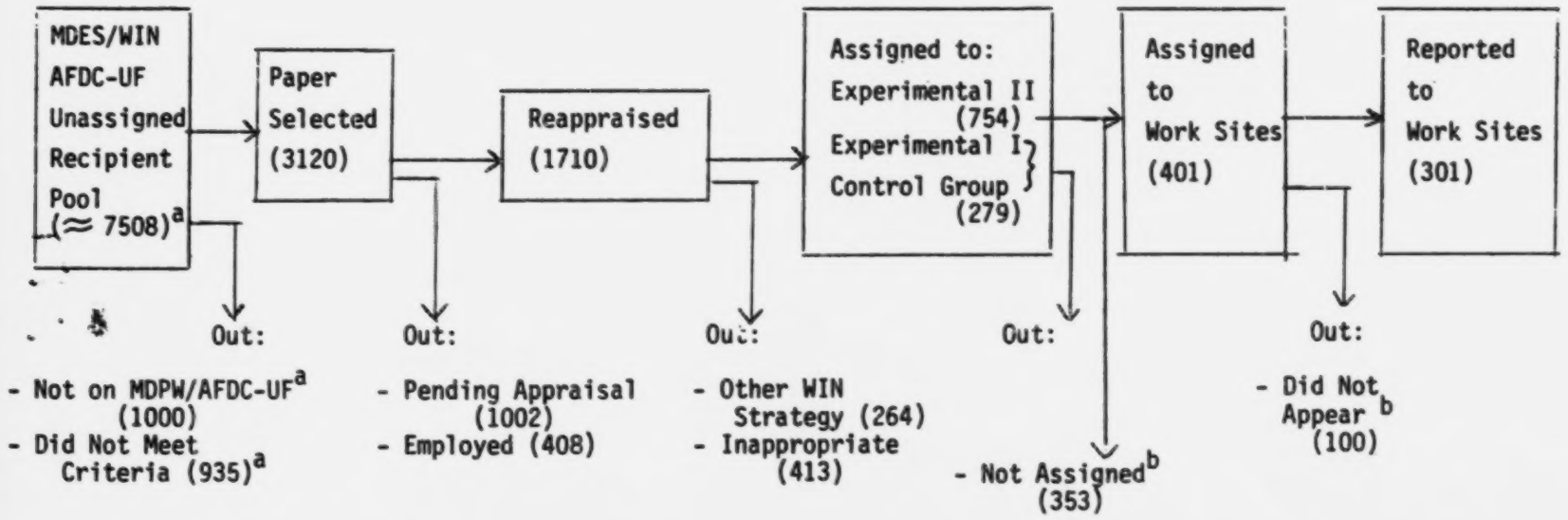
Since the MASSWEP program begins with the unassigned recipient pool, it is necessary to account for the size of the pool. This, however, depends on the overall AFDC-UF caseload.

1. The AFDC-UF Caseload

The AFDC-UF caseload as tracked by MDPW fell markedly during most of 1977, the year preceding the formal opening of MASSWEP on January 12, 1978. However, this caseload rose sharply towards the end of 1977. An attempt is made here to explain these fluctuations, although the study was not designed for this purpose. Since MDPW does not routinely compile vitally useful data on case openings and closings from its AFDC-UF records, the explanations offered must be viewed as somewhat speculative. Understanding fluctuations in AFDC-UF is further compounded by the fact that the same caseload as tracked by MDES moved in a direction opposite to that shown by the MDPW data; the MDES series first rose and then fell in 1977. The MDPW data are more reliable and it is to understanding these data that attention principally is directed below.

FIGURE D-1

MASSWEP IN STAGES: CLIENT FLOW FROM PAPER SELECTION THROUGH APPEARANCE AT WORK SITE



a/ With the exception of these three numbers, which are developed in section B of this chapter, all other data in the figure are taken from: MDES, "Summary Report of WIN/Work Experience Program through July 1979", August 10, 1979.

b/ This number is derived in section D of this chapter.

a. Disparities in MDES and MDPW AFDC-UF Caseloads

Before proceeding with a discussion of the factors causing changes in the AFDC-UF caseload in the period preceding MASSWEP, the sources of disparities in the records kept by MDES and MDPW on AFDC-UF should be addressed. In May 1977, MDES records had 9405 AFDC-UF registrants while MDPW was making payments to 5930 AFDC-UF cases. One source of the disparity is that AFDC-UF cases often are listed by MDPW as regular AFDC cases; payments are made to the mother but the father still has to register with MDES/WIN. This means that the MDPW data on AFDC-UF regularly understate the true caseload.

While MDPW records must be current to preclude payments to ineligible cases, MDES records need not be, another source of difference. Men finding jobs are deregistered from MDES/WIN with a deliberate lag of 30 to 60 days. In other instances, if a UF does not directly notify MDES/WIN that he has found a job, there is a time lag before this information is transmitted by MDPW to MDES/WIN. In addition MDES/WIN is slow to make adjustments in records when the status of a case changes, leading to MDES/WIN overstatement of the true welfare caseload. These lags in the record-keeping at MDES also make it possible for the same caseload as tracked by the two agencies to move in the opposite direction.

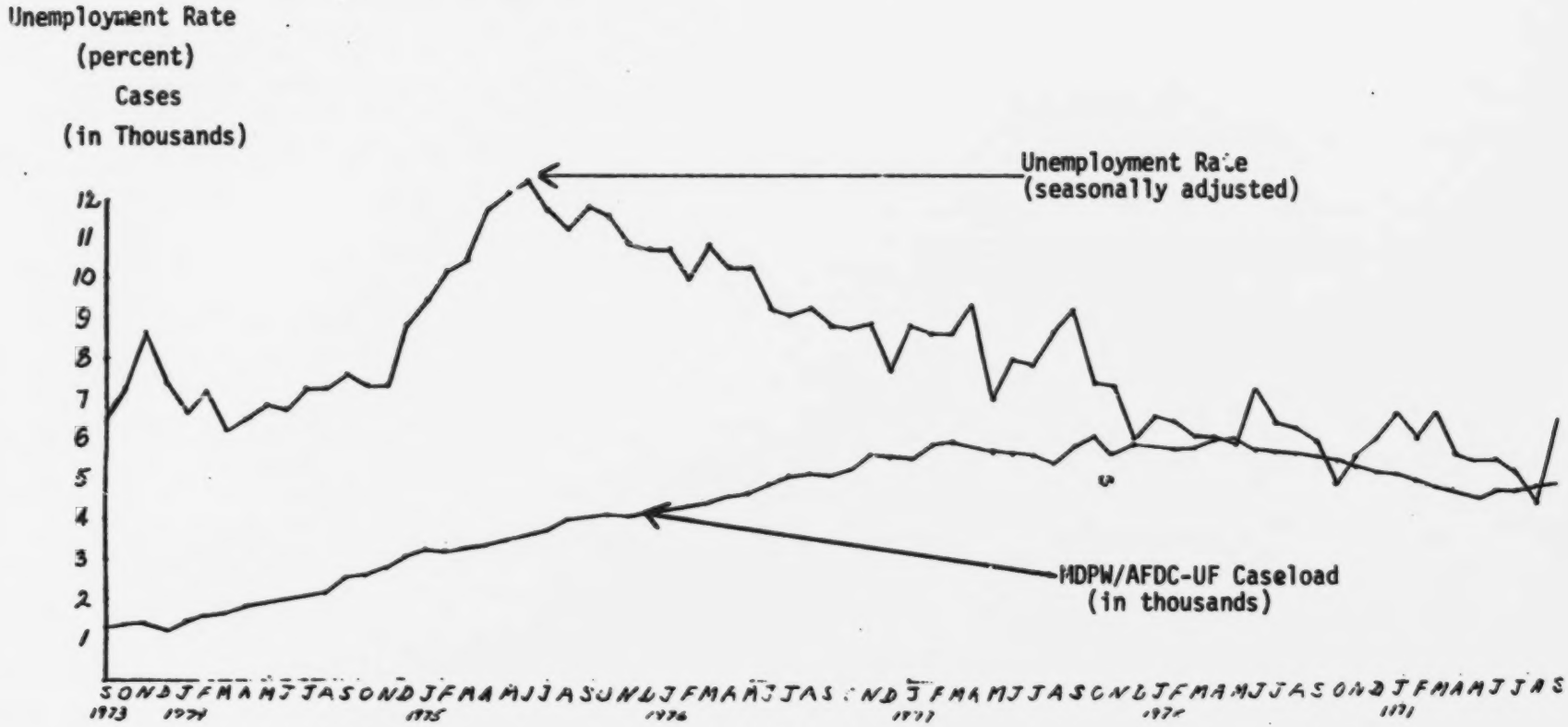
b. Changes in AFDC-UF Caseload

Probably the first factor contributing to the decline in the MDPW/AFDC-UF caseload through the fall of 1977 was the continuing improvement of the Massachusetts economy. From a peak for the 1970s of 11.8 percent in May 1975, the state unemployment rate continued to decline through 1975, 1976, 1977, reaching 5.5 percent by December 1977 (see Figure D-2). Fluctuations around this trend are observed, but its general downward direction is unmistakable and noteworthy. Throughout the last half of 1975 and all of 1976, the MDPW/AFDC-UF caseload continued to rise even as unemployment fell. The caseload then fell during

Figure D-2

Massachusetts Unemployment Rate and MDPW/AFDC-UF Caseload

By Months, September 1973-September 1979[a]



a/ Source: Federal Reserve Bank of Boston, New England Economic Indicators, Sept. 1979; Communication from Massachusetts Department of Public Welfare, Budget Office, December 28, 1979

the first three quarters of 1977. The MDPW/AFDC-UF caseload seems to respond to changes in the state economy with a long lag. This probably results from the fact that when unemployment is high and rising, people turn first for support to the Unemployment Insurance (UI) Program. When the MDPW/AFDC-UF caseload rose by an astonishing 11 percent between January and February of 1977, a case-by-case analysis of the increase revealed that almost all of it was attributable to the exhaustion of UI benefits.[1] However, analyses of the opening of new MDPW/AFDC-UF cases in August 1977 and in May 1978 indicate that by these months only 10 to 20 percent resulted from the exhaustion of UI.[2] These few pieces of information suggest a long lag: the MDPW/AFDC-UF caseload can rise long after unemployment reaches its peak; the caseload begins to fall only after the UI exhaustion effect is dissipated.

A factor accompanying declining unemployment and contributing to the eventual decline in the caseload was an effort in the WIN Program, begun in March 1977, to reduce the number of men on AFDC-UF through a special placement program channeling registrants into regular and CETA jobs. In the six month period between March 1, 1977 and August 31, 1977, 3445 AFDC-UF fathers were placed in regular jobs and1434 more found CETA jobs. Although data are not available to contrast these placement rates with those of earlier periods, they do compare favorably with data for the comparable period in 1978. In the latter six month period, 2847 entered unsubsidized jobs. It must be added, though, that the MDES/AFDC-UF caseload was about 10 percent lower during the 1978 period as contrasted with the 1977 period.

An administrative effort to "clean" the MDES/AFDC-UF files of closed cases also was launched in late 1977 in several MDES/WIN offices. Conducted

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[1] Alexander Sharp, Commissioner of MDPW, February Caseload Increases in AFDC-UF and AFDC, (Memorandum to Jerald Stevens, Secretary,) March 31, 1977.

[2] Donna Balboni, Survey of AFDC Case Openings in May 1978, MDPW Office of Research Evaluation, November 1978.

in anticipation of MASSWEP, this managerial activity also reduced the MDES/AFDC-UF caseload, though not that recorded by MDPW.

The joint product of declining unemployment and the WIN placement effort, combined with factors accounting for "normal" turnover in AFDC-UF, was substantial ferment in AFDC-UF in 1977, as recorded by both MDES and MDPW. The MDPW/AFDC-UF caseload, which rose from (roughly) 5500 cases in December 1976 to 6600 cases by March 1977, declined sharply to 5200 cases in August 1977, and then fluctuated up and down, ending the year at 5700 cases. The MDES/AFDC-UF caseload rose from 9400 cases in May 1977, the first month for which data were available from MDES/WIN, to 10,860 cases in September 1977 and then declined. Reported MDES/AFDC-UF deregistrations far exceeded registrations in October and November 1977, while the MDES/AFDC-UF caseload declined but slightly between October and December (see Table D-1). This is so in spite of the fact that not all registrants end up on AFDC-UF and thus in the caseloads of either MDES or MDPW. Some MDES/WIN registrants become employed before they are accepted for payments; others are rejected by MDPW. (Unfortunately, as noted above, data on case closings and case openings in AFDC-UF were not available to us through MDPW. Thus, changes in the MDPW/AFDC-UF Caseload cannot be described in terms of the relationship between those two variables.)

Lastly, it is useful to note the data in Table D-1 on the MDES/WIN unassigned recipient pool for AFDC-UF fathers. From its peak of about 7100 persons in June 1977, the unassigned recipient pool declined to 5700 in December 1977, a fall of 20 percent.

2. <u>The Cumulative Unassigned Recipient Pool Eligible for Paper Selection</u>

The pool of persons who were eligible for paper selection for MASSWEP is a <u>cumulative</u> one: it consists of those who were in the unassigned recipient pool in January 1978 plus those who entered it during

TABLE D-1

AFDC-UF Caseload

Month/Year	(1) MDPW Caseload (Number of Cases)	(2) MDES Caseload (Number of WIN Registrants)	(3) Number of New WIN Registrants	(4) Number of WIN Deregistrants	(5) Number of Entrants into Unsubsidized Jobs	(6) Number in Unassigned Recipient Pool
January '77	5,670	---[a]	---	---	---	---
February	6,270	---	---	---	---	---
March	6,587	---	---	---	---	---
April	6,280	---	---	---	---	---
May	5,930	9,405	810	1,098	587	6,396
June	5,648	10,154	871	971	525	7,093
July	5,425	10,005	894	1,047	425	6,488
August	5,200	10,497	1,497	1,005	419	6,636
September	5,528	10,866	1,142	773	347	6,962
October	5,583	10,191	826	1,500	529	6,023
November	5,262	9,700	847	1,338	421	5,630
December	5,706	9,770	961	891	291	5,705
January '78	5,955	9,319	856	1,407	345	5,228
February	6,142	9,406	763	887	284	4,987
March	6,388	9,435	1,028	1,001	456	4,738
April	6,490	9,253	686	868	544	4,594
May	6,150	8,578	602	1,277	624	4,280
June	5,718	8,003	583	1,158	499	3,866
July	5,540	7,619	600	984	360	3,578
August	5,383	7,545	779	853	364	3,384
September	5,241	7,329	540	754	328	3,344
October	5,078	7,332	590	1,000	416	3,579
November	4,946	7,194	606	744	397	3,553
December	5,002	7,092	690	815	288	3,616
January '79	5,140	7,149	931	778	374	3,590
February	5,245	7,373	730	785	329	3,048
March	5,255	7,227	698	759	395	2,927
April	4,948	6,895	562	931	394	2,635
May	4,563	6,768	705	723	410	2,387
June	4,574	6,497	609	929	340	2,288

[a] Data for these months are not available.

Sources: Column (1)--MDPW Caseload and Expenditure Report, Table 1.

Columns (2), (3), (4), (5)--MDES/WIN AFDC/UF Monthly Report.

Column (6)--MDES/WIN AFDC/UF Report: AFDC/UFs in the Unassigned Recipient Status (a worksheet).

the next thirteen months while MASSWEP was still screening the pool. In January 1978, 5228 AFDC-UF fathers were listed in MDES/WIN monthly reports as being in the pool.

To estimate properly the cumulative pool, one must estimate the number of persons who entered this category during the MASSWEP program and add that number to 5228. People entered the pool continually during the program. Unfortunately, although MDES/WIN keeps data on the number of new AFDC-UF registrants each month, it does not tally the number of persons entering the unassigned recipient pool. Men can enter the pool as new WIN registrants immediately after they become certified for AFDC-UF. They also may enter the pool after receiving some WIN treatment. During any spell on welfare, a given person could find himself two or more times in this pool. MDES/WIN does not monitor and record these flows. Each month it reports only the stock of persons in the pool.

A crude lower limit on the number of men added to the unassigned recipient pool after MASSWEP began can be obtained by recognizing that an average of 689 men registered for WIN every month during the first 13 months of the program. (See Table D-1, column 3, for January 1978 through February 1979.) During this period, the ratio of persons in the pool to total AFDC-UF WIN registrants fell from 56 percent to 50 percent. (See Table D-1, columns 2 and 6.) Whether this occurred because old registrants left the pool in large numbers or because few new registrants entered the pool is impossible to tell, again because these flows are not recorded by the WIN program. The sketchy data that are available from MDPW indicate that closings of AFDC-UF cases during 1978 slightly exceeded openings. The apparent steady in-flow of new cases to AFDC-UF suggests a similar in-flow of new cases to WIN. In fact, the WIN program recorded 9354 new AFDC-UF registrants for the first 13 months of MASSWEP. If it is assumed, to be very conservative, that 20 percent of this group entered the unassigned recipient pool at some point

during the time of their registration, then 1871 men should be added to the 5228 in the pool in January 1978. Also, some of the 4091 WIN registrants who were not in the pool in January 1978 may later have entered it as they completed receiving a WIN service. If 10 percent of this group later entered the pool, that adds 409 men to the cumulative pool.

These speculations provide an estimate of the cumulative unassigned recipient pool:

1.	Recorded unassigned recipient pool in January 1978	5228
2.	+ Estimated entries to pool during MASSWEP	1871
3.	+ Estimated WIN registrants in January 1978 who entered pool during MASSWEP	409
=	Cumulative unassigned recipient pool	7508

B. Paper Selection

Of the estimated 7508 men in the pool, 3120 were paper selected. What happened to the 4388 cases who were not? A review of the limited evidence suggests that it is unlikely that there was an intimidation effect at this stage of MASSWEP. If there was any, it probably was very limited. Most importantly, it is highly doubtful that the paper selection phase of MASSWEP could have induced any AFDC-UF father to alter his behavior, since selection largely took effect in the very first weeks of the program. During these weeks, clients were unlikely to know of the program. In any event, paper selection was an office procedure in the program from which they were supposed to be shielded. In a very few local WIN offices, clients inappropriately were advised during paper selection that MASSWEP was in effect. This may have caused some to scurry for medical exemptions, while others may have decided to find jobs, but this effect must have been limited.

Other possibilities can account for many - or even most - of the 4388. First, many men in the initial pool were not on MDPW/AFDC-UF at the time MASSWEP began. Second, many later entrants into the pool were unlikely to be men experiencing long-term unemployment. Third, normal turnover and regular

WIN activities suggest that many of these men were finding jobs at this stage even without intimidation. Fourth, MDES was learning of jobs already held, partly through normal processes, and partly perhaps through improvements in record keeping

a. Men in the Unassigned Recipient Pool but not on AFDC-UF

Many men were listed in the MDES/AFDC-UF caseload as recipients only because of inadequate record-keeping that failed to eliminate old cases. To reach an estimate of this group, start with the fact that 9319 men were listed as AFDC-UF WIN registrants in January 1978, a point when MDPW was making payments only to 5955 AFDC-UF cases. In addition to this group of 5955 AFDC-UF cases, there were many AFDC-UF cases listed in MDPW records as plain AFDC cases in the wife's name. A way of estimating the number of AFDC-UF cases listed as AFDC cases in presented by the fact that data collected for this project were matçhed with state AFDC-UF files. It was fou d that 20 percent of the AFDC-UF fathers in the Brandeis sample who were WIN registrants were not listed as having received AFDC-UF payments for the 15 months of MASSWEP or for six previous months. Since the Brandeis group knew that they were on welfare because by the reappraisal stage this had been verified, they had to be listed as being on AFDC if they were not on AFDC-UF. This finding justifies an inflation of the MDPW/AFDC-UF caseload of 5955 cases for January 1978 by one-fourth, yielding an estimate of 7500 AFDC-UF cases for that month. If the MDES/AFDC-UF caseload was 9319 and the true MDPW/AFDC-UF caseload was only 7500, then at least 1800 WIN registrants were not eligible for paper selection in January 1978 because they neither were on AFDC-UF nor AFDC.

It is difficult to estimate how many of these 1800 men were in the unassigned recipient pool of 5228 at MDES/WIN and how many simply were other types of WIN registrants, but a rough guess can be made. Since 56 percent of all WIN registrants in January 1978 were in the pool, probably at least that percentage of the 1800 cases came from that pool. Thus, we may specu-

ulate that 1000 of the 1800 WIN registrants who no longer were on welfare were erroneously included in the WIN pool in January 1978.

b. Men in the Unassigned Recipient Pool Who Did Not Meet Criteria for Paper Selection

Eighteen hundred and seventy one men entered the pool after January 1978. It seems plausible that many of these were either not unemployed for six months or not in the pool for three-months--both distinct possibilities because of their recent entrance to welfare. As a rough guess, it seems likely that at least half of these--or 935--were in the pool only on a short term basis and thus not suitable for paper selection.

Adding the 1000 and the 935 cases, 55 percent of the 4388 cases can be said, conservatively, to have been ineligible for paper selection. The qualitative discussion of paper selection in Chapter IV provides evidence of the frequent use of ad hoc criteria in eliminating cases at this stage. Whether the 45 percent of those not selected were erroneously or inappropriately omitted during paper selection, or whether some were intimidated into entering jobs cannot be conclusively established.

c. Men Who Found Jobs or Whose Jobs Were Revealed to MDES.

It is true that the data in Column 5 of Table D-1 shows entries into regular jobs among all WIN AFDC-UF registrants, not only those in the unassigned recipient pool. Included in this number might be some intimidated into accepting jobs by the introduction of MASSWEP. However, intimidation appears to be a minor cause, at best, of job finding. The number of jobs found grew steadily between February and May 1978, the period which coincided with perhaps half of the paper selections. However, the court injunction halting MASSWEP as a compulsory program took effect on March 2, 1978 and MASSWEP was resumed on May 15, 1978. Thus, the big surge in job entries took place when no intimidation was attempted in the whole program, let alone during paper selection. Once the injunction was lifted, job placements began to fall rather sharply.

Other factors besides intimidation can account for growth in the number of job entries. For example, between February and May 1979 there was an increase in placements among AFDC-UF clients, as there was in the same period in 1978. That observed in 1978 surely is greater than that for 1979, but the surge in placements during the spring may be suggestive of a simple seasonal effect.

C. Client Flows Between Paper Selection and Reappraisal

According to MASSWEP procedures, all 3120 AFDC-UF fathers who passed paper selection were to appear for reappraisals, but only 1710 or 55 percent actually did during the 15-month program (see Figure D-1).

Note, though, that of the 3120 men who were paper selected, 1002 still were "pending reappraisal" by the time the MASSWEP program ended. Most of the 1002 men never were called for reappraisal because their cases were not vigorously pursued or were simply abandoned after changes were made in MDES/WIN staff. It is even possible that some errors in paper selection also were detected among these cases. In a smaller number of cases, the hostility on the part of the WIN staff to MASSWEP resulted in their refusal to call in people for reappraisals. Yet others among the 1002 cases were not called in because SAU staff were unavailable for reappraisals. Thus, behavior on the part of MDES/WIN personnel resulted in 1002 cases, 30 percent of the 3120 fathers who were paper selected, not being called in for reappraisal.

These 1002 cases constitute 71 percent of the 1410 total cases who did not have a reappraisal. The remaining 408 cases became employed prior to reappraisal. At this step again, some of these may have found jobs in response to MASSWEP intimidation, but others found jobs as part of normal turnover. A very crude estimate of an intimidation effect at this stage can be ventured. First note that not all 408 men finding jobs necessarily had been called in for reappraisal. Given that most of the 1002 cases pending were not called in, it would not be unreasonable to guess that perhaps 30 percent of the 408 cases

were called in. If 30 percent had never been called in, then 123 of the 408 cases could not have been the objects of an intimidation effect.

Secondly, one has to compare the regular flow into jobs from AFDC-UF with that implied by the flow of the remaining 285 (=408-123) cases into jobs during the course of the program. The flow of persons in the Control Group into jobs following reappraisal, presumably reflecting the effects of "no treatment," can be used as a measure of this regular flow. This approach has its limitations: those of the 1710 clients who were reappraised and assigned eventually to the Control Group may not compare perfectly with the 3120 who were paper selected. Nevertheless, it is noteworthy that among those in the Control Group, roughtly 10 percent found jobs in the first quarter following reappraisal. If this is the normal or regular flow of men on AFDC-UF into jobs, then 10 percent, or 312, of the 3120 paper selected cases could have been expected to find jobs just in the first quarter following paper selection. (Flows in subsequent quarters should be ignored because flows from various subsets of the 3120 men are counted at subsequent stages of the program as flows into jobs.) The sum of this 312 and the previous 123 cases would more than account for the 408 cases that are of interest. Our speculations suggest that any intimidation effect at this stage would be small.

D. Reappraisal

The Brandeis data collection process tracks events starting with the reappraisal. Thus, the separation of AFDC-UF fathers into those appropriate and inappropriate for assignment to work sites was discussed in Chapter IV using data gathered by the Brandeis research group. This section completes the picture of client flows through MASSWEP for the stages of the program following the reappraisal.

Using state data, the reappraisal process yielded 1033 cases as appropriate for assignment to work sites out of a total of 1710 cases reappraised, or 60 percent. Among the 677 not appropriate, the state reports that 264 were assigned to some WIN component other than work experience. Unfortunately, state data do not report activities of the inappropriates other than assignment to other WIN alternatives.[3] In comparison to state data, Brandeis data from the "barrier" forms completed by MDES/WIN personnel is somewhat incomplete and perhaps not fully representative of inappropriates. However, the "barrier" forms show that of the men who were reappraised and deemed inappropriate, one-third was sent to other WIN components and one-third was not given further assistive treatment.

Following reappraisal, the random assignment process placed 279 of the men judged appropriate for assignment to work experience into the Control Group and Experimental Group I combined, while 754 were assigned to Experimental Group II according to state data.

E. Assignment and Appearance at Work Site

All men assigned to Experimental Group II were to be assigned to work experience sites, but assignments were actually made for only a fraction of these men. Based on data from time payment cards submitted by WIN teams, 256 men appeared at work sites; data from insurance forms gathered by WIN Central Staff suggest that up to 45 more appeared for very short periods, at most one day or less. In addition to this group of 301 men, another 60 men indicated in the Brandeis follow-up questionnaire that they had been asked to go to sites but did not; if this number is blown up to account for incomplete returns on the follow-up questionnaire, then as many as 100 men may have received assignments and not gone to sites. In all, then, of the 754 men in Experimental II, 401 (=256+45+100) men either appeared at sites or were asked to and did not.

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[3] The screening mechanism was supposed to identify clients for whom treatments other than work experience would be more suitable.

This leaves 353 (=754-401) who were never asked to go to work sites. If a man found a regular job on his own immediately, he would not have been sent to a work site. However, based on follow-up questionnaires, it appears that 129 who were never assigned to work sites also never found regular jobs.

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